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AN

ESSAY

ON

DEW,

AND

SEVERAL APPEARANCES

CONNECTED WITH IT;

BY

WILLIAM CHARLES WELLS, M.D. & F.R.S.

OF LONDON AND EDINBURGH.

The Second Edition.

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To JAMES DUNSMURE, Esquire,
Merchant in London.

MY DEAR SIR,

Without your aid, I should, in all probability, never have acquired the knowledge, upon which the following Essay is principally grounded; since I could not, I believe, have found any other place, considering that I was obliged to be daily in London, so well fitted for my experiments, as that which you permitted me to use during a very long time, though manifestly to the great inconvenience of yourself and your family. I beg leave to assure you, that I feel this kindness most strongly, and that my gratitude for it will never cease to exist.

I am,

My dear Sir,

Your most obedient Servant,

and faithful Friend,

WILLIAM CHARLES WELLS.

London,
August 25, 1814.

NOTICE CONCERNING THE PRESENT EDITION.

The infirm state of the Author's health having prevented him, since the publication of the former edition of his Essay on Dew, from making experiments in the open air during the night, and his reading having in the meanwhile been directed to other objects, the present edition of that Essay will be found to contain almost nothing more than the other. The chief difference between the two arises from a change in the form of several of his expressions. He has, for instance, altered the expression of 'saturation with moisture' to that of 'repletion with moisture' in order to avoid the appearance of maintaining, that common air is capable of dissolving water; a tenet unconnected with his theory. Sometimes he has subjoined to the phrase, which he now employs, on that subject, the words 'in a pellucid state;' when this addition has not been made, he wishes it to be understood.

ESSAY ON DEW,

&c.

INTRODUCTION.

I was led, in the autumn of 1784, by the event of a rude experiment, to think it probable, that the formation of dew is attended with the production of cold. In 1788, a paper on hoarfrost, by Mr. Patrick Wilson of Glasgow, was published in the first volume of the Transactions of the Royal Society of Edinburgh, by which it appeared, that this opinion had been entertained by that gentleman, before it had occurred to myself. In the course of the same year, Mr. Six of Canterbury mentioned in a paper communicated to the Royal Society, that, on clear and dewy nights, he always found the mercury lower in a thermometer laid upon the ground, in a meadow in his neighbourhood, than it was in a similar thermometer suspended in the air, 6 feet above the former; and that, upon one night, the difference amounted to 5° of Fahrenheit's scale. Mr. Six, however, did not suppose, agreeably to the opinion of Mr. Wilson and myself, that the cold was occasioned by the formation of dew;

but imagined, that it proceeded, partly from the low temperature of the air, through which the dew, already formed in the atmosphere, had descended, and partly from the evaporation of moisture from the ground, on which his thermometer had been placed. The conjecture of Mr. Wilson, and the observations of Mr. Six, together with many facts, which I afterwards learned in the course of reading, strengthened my opinion; but I made no attempt, before the autumn of 1811, to ascertain by experiment if it were just, though it had, in the mean time, almost daily occurred to my thoughts. Happening, in that season, to be in the country on a clear and calm night, I laid a thermometer upon grass wet with dew, and suspended a second, in the air, 2 feet above the other. An hour afterwards, the thermometer on the grass was found to be 8° lower, by Fahrenheit's division, than the one in the air. Similar results having been obtained from several similar experiments, made during the same autumn, I determined, in the next spring, to prosecute the subject with some degree of steadiness, and with this view went frequently to the house of one of my friends, who lives in Surrey. At the end of two months, I fancied that I had collected information worthy of being published; but fortunately, while preparing an account of it, I met, by accident, with a small posthumous work of Mr. Six, printed at Canterbury in 1794, in which are related differences observed on dewy nights, between thermometers placed upon grass and others in the air, that are much greater than those mentioned in the paper presented by him to the Royal Society in 1788. In this work, too, the cold of the grass is attributed, in agreement with the opinion of Mr. Wilson, altogether to the dew deposited upon it. The value of my own observations appearing to me now much diminished, though they embraced many points left untouched by Mr. Six, I gave up my intention of making them known. Shortly after, however, upon considering the subject more closely, I began to suspect, that Mr. Wilson, Mr. Six, and myself, had all committed an error, in regarding the cold, which accompanies dew, as an effect of the formation of that fluid. I, therefore, resumed my experiments, and having, by means of them, I think, not only established the justness of my suspicion, but ascertained the real cause both of dew, and of several other natural appearances, which have hitherto received no sufficient explanation, I venture now to submit, to the consideration of the learned, an account of some of my labours, without regard to the order of time, in which they were performed, and of various conclusions which may be drawn from them, mixed with facts and opinions already published by others.

PART I.

OF THE PHÆNOMENA OF DEW.

SECTION I.

Of circumstances which influence the production of Dew.

Aristotle* and many other writers have remarked, that dew appears only on calm and serene nights. The justness of this observation, however, has not been universally admitted. For Musschenbroek † says, that dew forms in Holland, while the surface of the country is covered with a low mist; but, as he mentions at the same time, that it is deposited upon all bodies indiscriminately, the moisture, of which he speaks, cannot properly be called dew, as will be more distinctly seen hereafter. Other writers of considerable reputation have also regarded clearness of the atmosphere, as not being requisite for the production of dew, misled, I believe, partly by theory, and partly by observing on

* Meteor. Lib. I. c. x. et De Mundo. c. iii.

† Nat. Phil. T. ii. De Rore.

misty mornings copious dews, which had been produced during preceeding clear nights. Respecting this point I can aver, after much experience, that I never knew dew to be abundant, except in serene weather. In regard to the necessity of the air being still, I know of no person who rejects it, except Mr. Prieur,* a late French author of little consideration, and he affirms, in opposition to the most common observation, that a fresh wind is requisite for the production of dew.

The remark of Aristotle, however, is not to be received in its strictest sense, as I have frequently found a small quantity of dew on grass, both on windy nights, if the sky was clear, or nearly so, and on cloudy nights, if there was no wind. If, indeed, the clouds were high, and the weather calm, I have sometimes seen on grass, though the sky was entirely hidden, no very inconsiderable quantity of dew. Again; according to my observation, entire stillness of the atmosphere is so far from being necessary for the formation of this fluid, that its quantity has seemed to me to be increased, by a very gentle motion in the air. Dew, however, has never been seen by me, on nights both cloudy and windy.

If, in the course of the night, the weather, from being calm and serene, should become windy and

* Journal de l'Ecole Polytechnique, Tom. ii. 409.

cloudy, not only will dew cease to form, but that, which has formed, will either disappear, or diminish considerably.

In calm weather, if the sky be partially covered with clouds, more dew will appear, than if it were entirely covered, but less than if it were entirely clear.

Dew probably begins, in this country, to appear upon grass, in places shaded from the sun, during clear and calm weather, soon after the heat of the atmosphere has declined. My opportunities, however, for making such observations have not been numerous, since, while pursuing this subject, I seldom went into the country, till late in the afternoon; but I have frequently felt grass moist, in dry weather, several hours before sunset. On the other hand, I have scarcely ever known dew to be present in such quantity upon grass, as to exhibit visible drops, before the sun was very near the horizon, or to be very copious, till some time after sunset. It also continues to form, in shaded places, after sunrise; but the interval between sunrise, and its ceasing to form, is, according to my observation, which, upon this point, has not been extensive, considerably shorter, than that between its first appearance in the afternoon, and sunset. Contrary, however, to what happens at sunset, if the weather be favourable, more dew forms a little before, and, in shaded places, a little after sunrise, than at any other time. Musschenbroek, therefore, errs greatly when he says, that dew does not form after the sun has risen. The preceeding observations, on the early appearance of dew in the afternoon, are to be restricted to what happens to grass, or other substances highly attractive of dew placed on the ground; for it occurs much later on similar substances, which are elevated a few feet above the ground, though upon these it continues to form, as long after the rising of the sun, as upon the others, if they be equally sheltered from the rays of that body.

The formation of dew, after it has once commenced, continues during the whole night, if the weather remain still and serene. Mr. Prieur, indeed, of whom I have already spoken, asserts, that dew forms only in the evening and morning, and that any which occurs in the former season always disappears in the course of the night. I can affirm, however, from long experience, that grass, after having been dewed in the evening, is never found dry until after sunrise, unless the weather has, in the mean time, changed. Upon one serene and still night, I placed fresh parcels of wool upon grass every hour, and by weighing each of them, after exposure for an hour, found, that they had all attracted dew.

When dew forms upon a smooth dense body as glass, and it is only by means of such a body, that the process can be accurately observed, the appearances are altogether similar to those, which occur on a like body, when exposed to the steam of water, a little warmer than itself. The exposed surface has first its lustre diminished, by a slight damp uniformly spread over it. As the moisture increases, it gathers into irregularly shaped flat drops, which are, at first, very small, but afterwards enlarge and run into one another, forming streamlets, by means of which a great part escapes from the body which had received it.

During nights, that are equally clear and calm, dew often appears in very unequal quantities, even after allowance has been made, for any difference in their lengths. One great source of these differences is very obvious. For, it being manifest, whatever theory be adopted concerning the immediate cause of dew, that the more replete the atmosphere is with moisture, previously to the operation of that cause, the more copious will the precipitation of water be, after this operation has commenced, all the circumstances, which tend to increase the quantity of moisture in the atmosphere, must likewise tend to increase the production of dew. Thus dew, in equally calm and clear nights, is more abundant shortly after rain, than during a long tract of dry weather. It is more abundant, also, throughout Europe, with perhaps a few exceptions, and in some parts of Asia and Africa, during southerly and westerly winds, than during those, which blow from the north and the east. Aristotle* says, that Pontus is the only country, in which dew is more copious during a northerly, than during a southerly wind. But a similar fact occurs in Egypt; for dew is scarcely ever observed there, except while the Etesian winds prevail. Both cases, however, though contrary to the letter, are consonant with the spirit of the rule; since the north wind, in one country, proceeds from the Euxine sea, and, in the other, from the Mediterranean. Another circumstance, of the same kind with the blowing of wind from the south and the west, as shewing that the air contains much moisture, is the lessening of the weight of the atmosphere. My experience on this point has not, indeed, been great, as the falling of the mercury in the barometer is very commonly attended with wind or clouds, both unfavourable to the production of dew; but still the greatest dew, I have ever witnessed, occurred while the barometer was sinking. A corresponding observation is made by Mr. de Luc, who says, that rain may be foretold, when dew is uncommonly abundant, in relation to the climate and season.†

To the greater or less quantity of moisture in the atmosphere, at the time of the action of the im-

* Meteor. Lib. 1. c. x.

† Rech. sur les Mod. de l'Atm. § 725.

mediate cause of dew, are likewise to be referred several other facts respecting its copiousness, the explanation of which is, perhaps, not so apparent, as in the preceding examples.

In the first place; dew is commonly more plentiful in spring and autumn, than in summer; the reason is, that a greater difference is generally found between the temperatures of the day and the night, in the former seasons of the year, than in the latter. In spring, this circumstance is prevented often from having a considerable effect, by the opposite influence of northerly and easterly winds; but, during still and serene nights in autumn, dew is almost always highly abundant.

In the second place; dew is always very copious, on those clear and calm nights, which are followed by misty or foggy mornings; the turbidness of the air in the morning shewing, that it must have contained, during the preceding night, a considerable quantity of moisture.

Thirdly; I have observed dew to be unusually plentiful on a clear morning, which had succeeded a cloudy night. For the air, having in the course of the night lost little or no moisture, was in the morning more charged with watery vapour, than it would have been, if the night had also been clear.

Fourthly; heat of the atmosphere, if other circumstances are favourable, which, according to my experience, they seldom are in this country, occasions a great formation of dew. For, as the power of the air, to retain watery vapour in a pellucid state, increases considerably faster, while its temperature is rising, than in proportion to the heat acquired, a decrease of its heat, in any small given quantity, during the night, must bring it, if the temperature be high, much nearer to the point of repletion, before it be acted upon by the immediate cause of dew, than if the temperature were low. We read, accordingly, in the writings of those, who have travelled into hot climates, of a copiousness of dew frequently observed by them there, which very much exceeds what occurs, at any time, in this country. But even here, dew, though for the most part scanty in our hottest season, is sometimes very abundant during it, an example of which occured to me on the night, common to the 29th and 30th of July 1813; for on that night, notwithstanding its shortness, more dew appeared, than has ever been observed by me on any other.

In the last place; I always found, when the clearness and stillness of the atmosphere were the same, that more dew was formed between midnight and sunrise, than between sunset and midnight, though the positive quantity of moisture in the air must have been less in the former, than in the latter time, in consequence of a previous precipitation of part of it. The reason, no doubt, is the cold of the atmosphere being greater in the latter, than in the prior part of the night.

But there are many circumstances, influencing the quantity of dew, which, though much more open to accurate observation, than those hitherto mentioned, are yet much less easy to be understood.

In my first attempts to compare the quantities of dew formed during different times, or in different situations, I attended only to the appearance, which it made on bodies having smooth surfaces. But quickly seeing this method to be very imperfect, I next employed wool to collect dew from the atmosphere, and found it well adapted for my purpose, as it readily admits amongst its fibres the moisture, which forms on its outer parts, and retains what it receives so firmly, that I never but once had occasion to suspect, that it suffered any portion of what it had thus acquired to pass entirely through it. The wool, which I used, was white, moderately fine, and already imbued with a little moisture, from having been long exposed to the air of a room, in which no fire was kept. I divided it into parcels of 10 grains each, and, immediately before exposure, pulled the fibres of every parcel somewhat asunder, so as to give it the form of a flattened sphere, the greatest diameter of which was about 2 inches. As in doing this, I went by the judgment of my sight alone, some little inequality, in point of size, must have existed among different parcels, but none, I think, sufficient to affect the accuracy of my conclusions from the experiments, in which they were employed, more especially as my conclusions scarcely ever rested upon single trials.

Previously to mentioning the results of any of my experiments with these parcels of wool, I think it right to describe the place, where by far the greater part of my observations on dew were made. This was a garden in Surrey, distant, by the public road, about three miles from the bridge over the Thames at Blackfriars, but not more than a mile and a quarter, from a densely built part of the suburbs on the south side of that river. The form of the garden was oblong, its extent nearly half an acre, and its surface level. At one end was a dwelling house of moderate size, at the other a range of low buildings; on one side a row of high trees, on the other a low fence, dividing it from another garden. If this fence had been absent, the garden would have been on the latter side entirely open. Within it were some fruit trees, but, as it had not been long made, their size was small. Towards one end, there was a grassplat, in length 62 feet, and nearly 16 broad, the herbage of which was kept short by frequent mowing. The rest of the garden was employed for the production of culinary vegetables. All of these circumstances, however trifling they may appear, had influence on my experiments, and most of them, as will hereafter be seen, must have rendered the results less remarkable, than they would have been, if they had occurred on a wide open plain, considerably distant from a large city.

I now proceed to relate the influence, which several differences in the situation, mechanical state, and real nature of bodies, have upon the production of dew.

I. One general fact relative to situation is, that whatever diminishes the view of the sky, as seen from the exposed body, occasions the quantity of dew, which is formed upon it, to be less than would have occurred, if the exposure to the sky had been complete.

I placed, on several clear and still nights, 10 grains of wool upon the middle of a painted board, $4\frac{1}{2}$ feet long, 2 feet wide, and 1 inch thick, elevated 4 feet above the grassplat, by means of 4 slender wooden props of equal height; and, at the same time, attached, loosely, 10 grains of wool to the middle of its underside. The two parcels were consequently only an inch asunder, and were equally exposed to the action of the air. Upon one night, however, I found, that the upper parcel had gained 14 grains in weight, but the lower only 4. On a second night, the quantities of moisture, acquired by like parcels of wool, in the same situations as in the first experiment, were 19 and 6 grains; on a third, 11 and 2; on a fourth, 20 and 4; the smaller quantity being always that, which was gained by the wool attached to the lower side of the board.

I bent a sheet of pasteboard into the shape of a house-roof, making the angle of flexure 90 degrees, and leaving both ends open. This was placed one evening, with its ridge uppermost, upon the same grassplat, in the direction of the wind, as well as this could be ascertained. I then laid 10 grains of wool on the middle of that part of the grass, which was sheltered by the roof, and the same quantity on another part of the grassplat fully exposed to the sky. In the morning, the sheltered wool was found to have increased in weight only 2 grains, but that, which had been exposed to the sky, 16 grains.

In these experiments, the view of the sky was almost entirely cut off from the situations, in which little dew was formed. In others, where it was less so, the quantity gained was greater. Thus, 10 grains of wool, placed upon the spot of the grassplat, which was directly under the middle of the raised board, and which enjoyed, therefore, a considerable oblique view of the sky, acquired during one night 7, during a second 9, and during a third 12 grains of moisture, while the quantities gained, during the same times, by equal parcels of wool, laid upon another part of the grassplat, which was entirely exposed to the heavens, were 10, 16, and 20 grains.

As no moisture, falling like rain from the atmosphere, could, on a calm night, have reached the wool in any of the situations, where little dew was formed, it may be thought, that the substances, under which the wool was placed, prevented, mechanically, the access of that fluid. But on this supposition it cannot be explained, why some dew was always found in the most sheltered places, and why a considerable quantity occurred upon the grass under the middle of the raised board. A still stronger proof of the want of justness in this supposition is afforded by the following experiment. I placed, upright, on the grassplat a hollow cylinder of baked clay, the height of which was $2\frac{1}{2}$ feet, and diameter 1 foot. On the grass, surrounded by the cylinder, were laid 10 grains of wool, which, in this situation, as there was not the least wind, would have received as much rain, as a like quantity of wool fully exposed to the sky. But the quantity of moisture, obtained by the wool surrounded by the cylinder, was only a little more than 2 grains, while that acquired by 10 grains of fully exposed wool was 16. This occurred on the night, during which the wool under the bent pasteboard gained only 2 grains of moisture.

Dew, however, will, in consequence of other varieties of situation, form in very different quantities, upon substances of the same kind, although these should be similarly exposed to the sky.

In the first place; it is requisite, for the most abundant formation of dew, that the substance attracting it should rest on a stable horizontal body of some extent. Thus, upon one night, while 10 grains of wool, laid upon the raised board, increased 20 grains in weight, an equal quantity, suspended in the open air, $5\frac{1}{2}$ feet above the ground, increased only 11 grains, notwithstanding that it presented a greater surface to the air than the other parcel. On another night, 10 grains of wool gained on the raised board 19 grains, but the same quantity suspended in the air, on a level with the board, only 13; and on a third, 10 grains of wool acquired, on the same board, $2\frac{1}{2}$ grains of weight, during the time in which other 10 grains, hung in the air, at the same height, acquired only $\frac{1}{2}$ a grain.

In the second place; the quantities of dew attracted by equal masses of wool, similarly exposed to the sky, and resting on equally stable and extended bodies, oftentimes vary considerably, in consequence of some difference in the other circumstances of these bodies. 10 grains of wool, for instance, having been placed upon the grassplat, on a dewy evening, 10 grains upon a gravel walk which bounded the grassplat, and 10 grains upon a bed of bare garden mould, immediately adjoining the gravel walk; in the morning, the wool on the grass was found to have increased 16 grains in weight, but that on the gravel walk only 9, and that on the garden mould only 8. On another night, during the time that 10 grains of wool, laid upon grass, acquired $2\frac{1}{2}$ grains of moisture, the same quantity gained only $\frac{1}{2}$ a grain upon the bed of garden mould, and a like quantity, placed upon the gravel walk, received no accession of weight whatever.

Two objections will probably be made against the accuracy of these, as well as my other experiments with wool. One is, that wool placed on grass may, by a kind of capillary attraction, receive dew previously formed on the grass, in addition to its own. To this I answer, that wool in a china saucer, placed on the grass, acquired very nearly as much weight, as an equal parcel immediately touching the grass. The second objection is, that a part of the increased weight in the wool might arise from its imbibing moisture, as a hygroscopic substance. I do not deny, that some weight was given to the wool in this way; but it may be safely affirmed, that this quantity must have been very small. For, on very cloudy nights, apparently the best fitted to increase the weight of hygroscopic substances, wool upon the raised board would, in the course of many hours, acquire little or no weight; and in London, I have never found 10 grains of wool, exposed to the air on the outside of one of my chamber windows, to increase, during a whole night, more than

$\frac{1}{2}$ a grain in weight. When this weight was gained, the weather was clear and still; if the weather was cloudy and windy, the wool received either less or no weight. This window is so situated, as to be, in great measure, deprived of the aspect of the sky.

It being shewn, that wool, though highly attractive of dew, was prevented, by the mere vicinity of a gravel walk, or a bed of garden mould, for only a small part of it actually touched those bodies, from acquiring nearly as much dew, as an equal parcel laid upon grass, it may be readily inferred, that little was formed upon themselves. In confirmation of this conclusion, I shall mention, that I never saw dew upon either of them. Another fact of the same kind is, that, while returning to London from the scene of my experiments about sunrise, I never observed, if the atmosphere was clear, the public road, or any stone pavement on the side of it, to be moistened with dew, though grass within a few feet of it, and painted doors and windows of houses not far from it, were frequently very wet. If, indeed, there was a foggy morning, after a clear and calm night, even the streets of London would sometimes be moist, though they had been dry the day before, and no rain had in the meanwhile fallen. This entire, or almost entire, freedom of certain situations from dew depends, however, much more upon extraneous circumstances, than upon the nature of the substances found there; for river sand, though of the same nature with gravel, when placed upon the raised board, or upon grass, attracted dew copiously.

A third difference, from situation, in the quantity of dew collected by similar bodies, similarly exposed to the sky, depends upon their position with respect to the ground. Thus, a substance placed several feet above the ground, though in this situation later dewed, than if it touched the earth, would, notwithstanding, if it lay upon a stable body of some extent, such as the raised board lately mentioned, acquire more dew during a very still night, than a similar substance lying on grass.

A fourth difference of this kind occurred among bodies placed on different parts of the raised board. For one, that was placed at the leeward end of it generally acquired more dew, than a similar body at the windward extremity.

II. Difference in the mechanical state of bodies, though all other circumstances be similar, has likewise an effect on the quantity of dew, which they attract. Thus, more dew is formed upon fine shavings of wood, than upon a thick piece of the same substance. It is chiefly for a similar reason, I believe, that fine raw silk, fine unwrought cotton, and flax, were found by me to attract somewhat more dew, than the wool I employed, the fibres of which were thicker, than those of the other substances just mentioned.

III. Bright metals, in consequence of some circumstance in their constitution, attract dew much less powerfully than other bodies; all of which, after allowance has been made for any difference, which may exist in their mechanical states, seem to attract dew in quantities not very unequal, if they be similarly situated.

Musschenbroek was the first, who distinctly remarked this peculiarity of metals; but Dufay,* I believe, published it before him, referring, at the same time, the discovery to its proper author. Both Muschenbroek and Dufay, however, made too large an inference from their experiments; for they asserted, that dew never appears on the upper surface of bright metals, whereas the contrary has since been observed by many persons, and I have myself known dew to form on gold, silver, copper, tin, platina, iron, steel, zinc, and lead. Dew, however, when it does form upon metals, commonly sullies only the lustre of their surface; and even when it is sufficiently abundant to gather into drops, these are almost always small and distinct. Two other facts of the same kind are; first, that the dew, which has formed upon a metal, will often disappear, while other substances in their neighbourhood remain wet; and secondly, that a metal, which has been purposely moistened, will often become

* Mem. de l'Acad. Fran. 1736.

dry, though similarly exposed with bodies which are attracting dew. This inaptitude to attract dew, in metals, is communicated to bodies of a very different nature, which touch or are near to them. For I have found, that wool laid upon a metal will acquire much less dew, than an equal quantity laid upon grass in the immediate vicinity.

A large metallic plate, lying on grass, resists the formation of dew more powerfully than a very small one similarly situated. I conclude from various collateral facts, that a considerable difference in the thickness of two pieces of metal, exposing equal surfaces to the sky, will be attended with a similar consequence, wherever they be placed, though I have no observation, which proves this directly. If, however, a large and a very small plate be suspended horizontally, at the same height, in the air, the small plate will resist the formation of dew more powerfully than the large.

If a metal be closely attached to a substance of some thickness, which attracts dew powerfully, the attraction of the metal itself for dew, instead of being increased from this circumstance, becomes diminished, provided the metal cover the whole of the upper surface of the other body. If only a part of this body be covered, the production of dew on the metal is forwarded by the conjunction, and this somewhat in proportion, to the quantity of surface in the lower body left uncovered. The justness of the first of these observations is proved by the following experiment. I joined, in the form of a cross, two pieces of very light wood, each 4 inches long, a third of an inch in breadth, and 1 line in thickness. To one side of this cross I fastened, by means of mucilage, a square piece of gilt paper, and then exposed the instrument to the sky, with its metallic side uppermost, on a dewy night, by suspending it, in a horizontal position, about 6 inches above the ground. A few hours after, the unattached parts of the metalled paper were found covered with minute drops of dew, while those, which adhered to the cross, were dry.

A large metallic plate, laid upon grass, was dewed with more difficulty on its upper surface, than a similar plate elevated a few inches above the grass, by means of slender props, which allowed the air to pass freely under the metal. But the case with respect to small pieces was the reverse; for I have often seen, covered with dew, the metallic sheath of a small thermometer lying upon grass, while the similar sheath of another thermometer, suspended in the air, remained dry.

Removing a metal several times, in the course of the night, from one part of the grassplat to another, facilitated its being dewed. The same effect was produced on gilt and silvered paper, by first exposing them to the sky, for some time, with the bare side uppermost, and then turning them.

If a piece of glass, covered on one side with a metal, be placed upon the ground, with this side downwards, the upper surface will attract dew, precisely as if no metal were attached to the lower surface.

The upper surfaces of metals are most readily, and most copiously dewed, on those nights, and in those parts of the night, during which other substances are the most readily, and the most copiously dewed.

If a metallic plate had been laid upon grass, before dew began to form anywhere, its lower side, notwithstanding, always became moist in the course of the night; and the same effect was almost always observed, if the plate had been placed horizontally in the air, a few inches above the grass. While the undersides were thus moist, the upper surfaces were very often dry. If, however, the plate was elevated several feet in the air, the condition of both sides was always the same, whether this was dry or moist.

The remarks hitherto made, on the relation of metals to dew, apply to the class generally; but it is now to be mentioned, that they do not all resist the formation of that fluid, with the same force.

I saw, for example, platina one night distinctly dewed, while gold, silver, copper and tin, though similarly situated, were entirely dry; and I have also several times seen these four metals free from dew, while iron, steel, zinc, and lead were covered with it.

I once supposed, in consequence of the difficulty with which metals are dewed, that they might in all circumstances resist, in a greater degree than other bodies, the condensation of watery vapour upon their surface; and I afterwards found, that Le Roi* asserts this to be the case. But, having exposed at the same time, to the steam of warm water, pieces of glass and of metal, I did not see, that moisture formed in the least more readily, upon the former than upon the latter. I have since learned, that Saussure † once entertained a similar suspicion, which was also proved by an experiment to be groundless.

All my experiments, hitherto spoken of, were made in the country. But Le Roi having said, that dew is never deposited by the air of cities, I determined to ascertain, if his assertion was just. With this view, I frequently exposed, at night, 10 grains of wool upon a slight wooden frame, placed in such a manner, between two ridges of the top of my house, which is situated in one of the most crouded districts of London, as to be 3 feet distant

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* Mem. de l'Acad. Fran. 1751.

† Hygronometrie, p. 329.

from the nearest part of the roof. The event was, that, upon clear and calm nights, dew was always acquired by the wool, though never in any considerable quantity; probably, however, more from the wooden frame being nearly surrounded by buildings, much more elevated than itself, than from any particular condition of the air in cities. The formation of dew, in this situation, proceeded much less regularly than in the country. For, upon one evening, 10 grains of wool gained in it 3 grains of moisture, in 1 hour and 18 minutes, though I scarcely ever knew a greater quantity to be collected by a similar parcel of wool, in the same place, during a whole night. These experiments will no doubt seem to many superfluous, since dew may be observed every fine evening, upon grass in London. But as dew upon grass is said by Le Roi to proceed from the ground, and not from the atmosphere, the argument derived from its appearance there, in cities, against his assertion is thus eluded by him.

The last subject, which I shall here touch upon, is that of hoarfrost.

This substance has, I believe, from the time of Aristotle,* been uniformly, and, according to my observations, justly, considered as frozen dew. I shall, therefore, frequently refer hereafter to the

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* Meteor. Lib. I. c. x.

experiments of the late Mr. Patrick Wilson of Glasgow respecting it, as if they had been actually made upon that fluid. Indeed, several of my experiments upon dew were only imitations of some, which had been previously made upon hoarfrost, by that ingenious and most worthy man.

SECTION II.

Of the Cold connected with the formation of Dew.

Dew is often spoken of as being cold, by popular writers. Thus Cicero and Virgil apply to it the epithet of 'gelidus,' Milton that of 'chill,' and Collins that of 'cold.' Of the same import is a passage in Herodotus, in which it is said, that, in Egypt, the crocodile passes a great part of the day on dry land; but the whole of the night in the Nile, this being warmer than the atmosphere, and the *dew*. Among philosophers, however, Mr. Wilson was the first, I believe, who ever suspected the existence of such a conjunction.

In my experiments on the temperature of bodies moistened with dew, small thermometers were employed, (the largest being only 8 inches long) having globular bulbs, which, in most of them, were not more than from 2 to $2\frac{1}{2}$ lines in diameter. Their scales, which were marked in the manner of Fahrenheit, were of ivory or wood, and were furnished, almost all of them, with hinges. They were always employed naked, except I wished to know the effect of covering them with any particular substance.

By means of these instruments I have very many times, during serene and still nights, examined the temperature of dewed grass, and have constantly observed it to be less than that of the air, anywhere between 1 inch and 9 feet above the ground, the latter being the greatest height, at which I ever marked the heat of the atmosphere, in these experiments. I generally, however, compared the temperature of dewed grass with that of the air 4 feet above the ground; and on nights, that were calm and clear, very frequently found the grass, at the ordinary place of my observations, 7, 8, or 9 degrees colder than the air at that height. Several times it was 10° and 11° colder than the air, and once 12°. These differences are not so great, as those related in Mr. Six's posthumous work. But, in his experiments, the temperature of grass was compared with that of the air 7 feet above the ground, which, in clear and calm nights, may be regarded as $\frac{1}{2}$ a degree warmer than the air at the height of 4 feet.

Besides; the most considerable differences, mentioned by Mr. Six, occurred in winter, when he says a greater degree of cold is occasioned by dew, than at any other time; whereas very few of my experiments, on the temperature of grass, were instituted in that season. In the last place; my experiments were almost always made on very short grass, while Mr. Six's thermometers were laid upon long grass bent, by strong pressure, towards the earth; in which state they marked a temperature 1, 2, and 3 degrees lower, than that shewn by similar thermometers placed upon grass, less than an inch in height. Had it not been for these circumstances, and the unfitness, in various respects, besides the shortness of the grass, for the production of a great cold, of the common scene of my operations, I believe that, in consequence of my thermometers being much better adapted to mark a superficial, or transitory cold, than those of Mr. Six, I should at some time have seen a difference several degrees greater, than the greatest ever seen by that gentleman, which was one of $13\frac{1}{2}°$. In confirmation of this opinion, I shall mention, that having, during a short visit to a more distant part of the country, exposed, in the evening, a thermometer upon the surface of an open grass field, I found it soon after, although the grass was short, and the weather warm, 14° lower than a similar thermometer, suspended in the air, 4 feet above the grass. If to this quantity be added $\frac{1}{2}$ a degree, on account of the difference in elevation between our suspended thermometers, the cold, connected with dew, observed by me this night on grass, will exceed the greatest ever observed by Mr. Six by 1 degree.

According to a few observations made by me, the greater coldness of grass, than that of the air, begins to appear, in clear and calm weather, in places, sheltered in the afternoon from the sun, but still open to a considerable portion of the sky, soon after the heat of the atmosphere has declined. A similar coldness continues upon grass in still and serene mornings, for some time after the rising of the sun, in places shaded from its direct light, but otherwise open to the sky. My experiments on this point have also not been many, and none of them were made in winter; which, I presume are the reasons, that I never observed a cold, from this cause, later in the morning, than an hour after sunrise. The surface of snow, however, was once, in the depth of winter, observed by Mr. Wilson of Glasgow to be considerably colder than the air, till a little after midday.*

In cloudy nights, particularly if there was wind, the grass was never much colder than the air. On such nights, the temperatures of both were

* Paper in Phil. Trans. 1781.

sometimes the same; at other times that of the grass was the higher of the two, even when the grass was wet from preceding rain, and when, consequently, it must have been, in some measure, cooled by evaporation. On one such night, the grass was found to be 4° colder than the earth an inch beneath the surface of the plat, which afforded a sufficient reason for the grass itself being warmer than the air. In windy weather, however, if the sky was clear, some degree of cold, in addition to that of the air, was always observed upon the grass; and in calm weather, very high clouds, though sufficiently extensive and dense, to conceal the sky completely, would yet frequently allow of the grass being several degrees colder than the air. I once observed, upon a night of this kind, a difference of 5° between the temperatures of those bodies.

If the night became cloudy, after having been very clear, though there might be no change with respect to calmness, a considerable alteration in the temperature of the grass always ensued; and this sometimes very suddenly. Upon one such night, the grass, after having been 12° colder than the air, became only 2° colder than it, the temperature of the air being the same at both observations. On a second night, grass became 9° warmer in the space of an hour and a half. On a third night, in less than 45 minutes, for the whole change occurred while I was absent 45 minutes, the temperature of the grass rose 15°, while that of the neighbouring air increased $3\frac{1}{2}$°. During a fourth night, the temperature of the grass at half past 9 o'clock was 32°. In 20 minutes afterwards, it was found to be 39°, the sky having in the mean time become cloudy. At the end of 20 minutes more, the sky being clear, the temperature of the grass was again 32°. These were the most remarkable of my observations on this subject; but I may add to them, that I have frequently seen, during nights that were generally clear, a thermometer lying on the grassplat rise several degrees, upon the zenith being occupied only a few minutes by a cloud. On the other hand, upon two nights I observed a very great degree of cold to occur on the ground, in addition to that of the atmosphere, during short intervals of clearness of sky, between very cloudy states of it.

I did not speak in the preceding section of another obscure state of the atmosphere, that occasioned by fog, or mist, as the moisture deposited in it attaches to all bodies, indiscriminately; on which account, I was unable to determine, whether or not dew forms during its continuance. But, with respect to the connexion of this condition of the atmosphere with cold, I have to remark, that I have several times, on its appearance betwixt daybreak and sunrise, found the difference between thermometers on grass and in the air, which had been considerable during the night, to diminish greatly. I never, indeed, observed it to vanish, but this I used to impute to the air being not very much obscured. I have now, however, reason to doubt the justness of this conclusion; for on the evening of the 1st of January in the present year, 1814, I found, during a dense fog, while the weather was very calm, a thermometer lying on grass, thickly covered with hoarfrost, 9° lower than another suspended in the air, 4 feet above the former. On the following evening, when the air was equally calm, but the fog sufficiently attenuated to allow me to see, that the sky was almost entirely covered with clouds, the difference between two thermometers, similarly placed with the former, was only 1°. On comparing the observations of these two evenings, I conclude, that on the first few or no clouds existed above the fog, and consequently that fog, if there be no clouds above it, may, in a very calm air, admit of the appearance of a considerable degree of cold, at night, upon the surface of the earth, in addition to that of the atmosphere. Mr. Six, indeed, says, while speaking of the cold connected with dew, in his paper in the Philosophical Transactions for 1788, "fogs did not, as far as I could perceive, at all impede, but rather increase, the refrigeration." But this was a mistake; which in all probability arose from his ascribing the effect of a clear night to an ensuing foggy morning, as he examined his thermometers only in the day time. He afterwards discovered his error; for, in his posthumous work, thick fogs are ranked among the circumstances, which always impede, and sometimes prevent altogether, the appearance of a cold upon the surface of the earth, greater than that of the atmosphere. During a very dense fog, Mr. Wilson found no difference, at night, between a thermometer laid upon snow, and another suspended in the air.*

When, during a clear and still night, different thermometers were examined, at the same time, which had been placed in different situations, those which were situated, where most dew was formed, were always found to be the lowest. Thus, upon one such night, I found a thermometer placed upon a little wool, lying upon the middle of the upper side of the raised board, to be 9° lower than another thermometer, in contact with an equal quantity of wool, attached to the middle of the underside of the board. On two other nights, the difference between two thermometers in the same situations was 8°. I found also, on two other serene and calm nights, a spot of grass covered by the pasteboard roof, and another spot surrounded

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* Edin. Phil. Trans. I. 170.

by the earthen cylinder, to be both 10° warmer than neighbouring grass fully exposed to the sky. Thinking it possible, that the cylinder, which had been exposed to the sun the preceding day, might still possess some of the heat, which it had then imbibed, I placed near to it, on another night, a cylinder made of very thin pasteboard; but this was equally efficacious with the earthen one, in preventing cold from occuring on grass. When the exposure was greater than in the preceding examples, and more dew was in consequence formed, the cold was also greater, but still less than where the exposure was complete. For instance, upon the night during which 10 grains of wool, placed upon the middle of the grass, which was sheltered by the raised board, had gained 7 grains, and the same quantity on grass fully exposed to the sky had gained 10 grains, the difference between the temperatures of the two portions of grass was only $2\frac{1}{2}$°.

The same correspondence was observed, when the differences in the quantity of dew did not depend, as in the preceding instances, upon any diversity of exposure to the sky. Thus, the mercury in a thermometer placed upon wool, lying on the raised board, was found to be at the 44th degree, while that in another, pendent in the air, at the same height from the ground, and wrapped in wool, was at the 48th. Wool also, on the raised board,* was commonly a little colder than the same substance on grass, when the night was very still; and the leeward end of that board was generally colder than the windward extremity.

But, the most remarkable examples of this kind were exhibited by the gravel walk, and the bare garden mould. In still and serene nights, the surfaces of these bodies were always warmer than the neighbouring grass, and frequently warmer than the air. On one night of this description, I observed, $2\frac{1}{2}$ hours after sunset, the surface of the gravel walk to be $16\frac{1}{2}°$, and that of the garden mould to be $12\frac{1}{2}°$, warmer than grass very near to them, and similarly exposed to the heavens. As the night proceeded, clouds formed and accumulated; in consequence of which the difference at sunrise, between the temperatures of the grass and the gravel walk, was only 6°, and between those of the grass and the mould only 4°, the temperature of the grass having in the mean time in-

* The greater cold of the raised board, in my experiments, most probably depended on the grass being very short; since Mr. Wilson found, that snow on the ground was colder than the same body on a raised board. If 1, 2, or 3 degrees were added to the cold of the grass at my place of observation, agreeably to the difference found by Mr. Six, between the temperatures of long and short grass in dewy nights, the cold on my raised board would, upon such nights, have been always less than that of the grassplat.

creased considerably, while that of the other bodies had decreased a little. At another time, shortly before sunrise, a very clear morning having succeeded a cloudy night, I found the gravel walk to be 10° and the garden bed to be 9° warmer than neighbouring grass, which was 8° colder than the air. Both of these examples occurred in summer, and I believe, that such considerable differences will occur in that season only. It was on the first of these two nights, that 10 grains of wool gained only ½ a grain of moisture on the mould, and that the same quantity gained no weight on the gravel walk. That the unfitness of the gravel walk, however, to become cold, like its unfitness to attract dew, arose from its situation, and not from the nature of the substance of which it was made, is proved by this cicumstance, that river sand, placed on the raised board, was on 4 different nights, none of them highly favourable for the production of cold, 7, 7, 8, and 8½ degrees colder than the air at the same height.

It may be added here, that I have always found, on dewy nights, the temperature of the earth, ½ an inch or an inch beneath its surface, much warmer than the grass upon it. On five such nights the differences were from 12 to 16 degrees. The earth, at the above-mentioned depth, was also almost constantly warmer on dewy nights than the air; sometimes it was considerably so, for I once observed it to be 10° warmer, at another time 9° and at a third $7\frac{1}{2}$°. An exception will no doubt occur, if very mild weather should follow a long frost; but of this I have had no experience.

In the experiments upon my housetop in London, I always found, during clear and calm nights, wool lying on the wooden frame to be colder than the air, at the same height; but the difference was seldom more than 3°. On the evening, however, during which dew formed there more copiously than usual, the difference was 5°. That the smallness of these differences was not wholly occasioned by any thing special in the air of cities was afterwards proved, by my finding others much greater, in a garden nearly in the middle of London, from which almost the whole of the sky was visible.

Metals, likewise, furnish proofs of the connexion of dew with a cold in the substance, on which it forms, superior to that of the neighbouring atmosphere. My observations, however, on the temperature of metals, when exposed to the sky on dewy nights, were less numerous, than those on several other subjects treated in this Essay, by reason of the less frequent opportunity I enjoyed of making them; and many of those, which I did make, were afterwards found by me to have been improperly conducted. I thought, for instance, for some time, that the temperature of a metal, on a dewy night, might easily be learned in the way, in which I had been accustomed to ascertain the temperature of dewed grass. But, observing dew one night on the glass tube of a thermometer, which was lying on a metal placed upon grass, while the metal itself was free from moisture, I conceived it probable, that the cold then indicated by the thermometer was not the real temperature of the body, to which it was applied. To determine the point, I placed on the same metal a second thermometer, covered with gilt paper, upon which this was found at three observations to be $6\frac{1}{2}°$, $7°$, and $7°$ higher than the other. In this experiment, the bulb of the naked thermometer, from being very small, did not project as far as the outer surface of the scale, and, consequently, did not come in contact with the metal. But even when the ball of a thermometer was applied directly to a metal, on a clear and calm night, a temperature was marked by it, commonly 2 and 3, and sometimes more degrees less than that marked by a similar thermometer, inclosed in gilt paper, and similarly placed. I found it likewise necessary, in this inquiry, to correct the temperature of the air, as given by a naked thermometer. For, on still and serene nights, a thermometer inclosed in a case of gilt or silvered paper, and suspended in the air 4 feet above the grass-plat, was usually observed to be $1\frac{1}{2}°$ or $2°$ higher than a bare thermometer, of the same construction, suspended near to it. The difference of two such thermometers, thus placed, was once observed by me to be $2\frac{1}{2}°$, and once $3\frac{1}{2}°$. It may be thought, perhaps, that these differences were caused by the metalled case obstructing the transmission of the temperature of the air to the inclosed instrument. But that this was not the reason is shewn by my observing, that on cloudy nights there existed no difference between the two thermometers; that, even on clear nights, a thermometer contained in a case of white paper, somewhat thicker than the metalled, was always nearly of the same temperature with a naked one which was suspended close to it; and that, when a difference did exist between the two latter, the thermometer in the white paper case was commonly lower than the other.

The estimation of the heat, both of air and of metals, on a dewy night, is liable to errors from other causes. As these, however, are trifling, I shall not mention them, but proceed to state the results of my observations, upon the temperature of metals exposed to the sky at night, though unable to vouch for their entire accuracy.

Thin bright metallic plates, the least having a surface of 25 square inches, and some of them a surface of more than 100 such inches, were several times observed, while lying on grass which was attracting dew, to be 1 and 2, and once 3, degrees warmer than the air 4 feet above them. At other times, their temperature was the same with that of the air. In both of these cases their upper surfaces were always free from dew. Metals thus situated were, consequently, often much warmer than the grass, which surrounded them. I made no experiments on this point, during the nights, on which occurred the greatest instances of cold on grass, relatively to the temperature of the air; but I found, notwithstanding, during one night, a metal on grass to be 10° warmer than the exposed grass near to it. On two other nights, the differences were 9° and 8°. The superiority of the heat of metals on grass over that of the air, when it did exist, was evidently connected with the temperature of the grass, which they covered, and this again with that of the earth under the same portion of grass; for this portion was always a little warmer than the metal, but not so warm as the earth.

On the other hand, metals, on which dew was forming while they lay upon grass, were always colder than the air. In like manner, if one metal upon the grassplat were dewed, while another similarly situated remained dry, the former was always colder than the latter.

When a metal lying on the grassplat became dewed, the grass under it was always colder than that under another metal, which was undewed.

A metal, while receiving dew, in consequence of being elevated in the air, was always colder than a similar metal, which remained undewed on the grass.

The greatest instances of cold, observed by me on metals, occurred at times, when other bodies near to them had become considerably colder than the atmosphere.

The cold, however, contracted by metals, from exposure to the sky in a clear and still night, was always less than that of other bodies similarly situated, the greatest excess of cold ever observed by me, in the larger metallic plates, from this cause, over that of the air, being not more than 3 or 4 degrees. If much smaller pieces were placed upon grass, the result was different. For I have found a small thermometer placed in this situation, while inclosed in a sheath of gilt paper, to be only 3° less cold than the surrounding grass, during a night favourable to the production of cold on the surface of the earth.

I collected only a few facts respecting the comparative temperatures of different metals, when they were exposed together to the sky, on dewy nights; but such as I did collect tend to prove, that the most readily dewed metals become colder than the air, sooner than those, which receive dew with greater difficulty.

Many of the experiments, which have been mentioned in this section, shew, that when bodies, which had been equally exposed to the night air, were examined at the same time, those which were most dewed were also the coldest. No such correspondence, however, was found in the experiments of different nights, or even of different parts of the same night. Thus, during two nights, on which grass was 12° and 14° colder than the air, there was little dew; while on the night, which afforded the most copious dew ever observed by me, the cold possessed by the grass, beyond that of the air, was for the most part only 3° and 4°; and I have always seen less dew about sunset, than about sunrise, when the weather has been calm and clear at both times, though there is commonly, in this country at least, a greater difference between the temperature of grass and of air in the evening, than in the morning. I had early observed, also, bodies exposed to the sky, on a cloudy but calm night, to be sometimes 2° or 3° colder than the air, without having any appearance of dew; and when two metals possessing different relations to dew were exposed together, I have seen the one, which was the fitter to attract that fluid, colder than the other, though both were dry.

I shall conclude this part of my Essay, with relating the results of some experiments, which were made for the purpose of ascertaining the tendencies of various bodies to become cold, upon exposure to the sky at night. Unfortunately, the weather was not always favourable to my views; but what occurred appears to me, notwithstanding, worthy of being related.

In the observations hitherto given by me on the cold connected with dew, the temperature of grass has been chiefly considered, partly because my first experiments had been made upon it, and partly from a wish, which arose afterwards, to compare my own experiments with those of Mr. Six, which had been confined to that substance. I found it, however, very unfit to furnish the means of comparing the degrees of cold produced at night on the surface of the earth, at different times and places; as its state on different nights, on the same parts of the plat I commonly made use of, and in different parts of the plat on the same nights, was often very unequal, in point of height, thickness and fineness, all of which circumstances influenced the degree of cold produced by it. I observed, in consequence, a much greater uniformity in the results of experiments made with various other bodies, whose condition, when first exposed to the air, was always the same. Of these, the most productive of cold were the filamentous and downy, as wool of moderate fineness, very fine raw silk, very fine unspun cotton, fine flax, and swandown, all of which were not only more steadily cold, upon clear and calm nights, than grass, but also gave rise to a greater degree of cold, than was almost at any time observed upon it, even in its best state. Among the bodies of this clsss, wool produced the least cold, and I formerly mentioned that it attracted less dew, than silk, cotton, and flax. The last mentioned substances, and swandown, were found equal, or nearly so, in their tendency to become cold. Swandown, however, exhibited the greatest cold rather more frequently than any of the rest; on which account, and from its being more easily managed, as it was used while adhering to the skin of the bird, I at length scarcely ever employed any other body of the same class. On the night, during which grass was observed to be 14° colder than the air, swandown, lying upon a neighbouring piece of grass, was still one degree lower. This difference of 15°, between the temperature, at night, of a body on the surface of the earth, and that of the air, a few feet above the earth, is the greatest which I have hitherto seen.

Fresh, unbroken straw, and shreds of white paper, though not properly to be ranked among filamentous substances, were also found to be a little more productive of cold, than the wool which I used.

The next class consisted of bodies in the state of a powder, more or less fine. These were clean river sand, glass, chalk, charcoal, lampblack, and a brown calx of iron. Chalk produced the least, and the three last substances, the greatest cold. They were all, however, inferior in this respect to bodies of the first class.

Solid bodies, having a surface exposed to the sky, of at least 25 inches square, formed a third class, on which such experiments were made. The particular substances of this description, subjected to trial, were glass, brick, cork, oakwood, and wax; all of which were, likewise, found inferior to the filamentous substances. From these last experiments it follows, that when a glass bulb of a thermometer is applied at night to a body exposed to a clear sky, the temperature exhibited by the instrument will not be accurately that of the body in question, except the disposition of the latter to become cold, in such a situation, be the same as that of glass. An example of this fact was given in the 39th page of this Essay.

My principal experiments, however, of this kind were made with snow.

On the 25th of January 1813, the ground being then covered with snow about an inch deep, I went to my usual place of experiment in the country; but, during 8 hours that I attended to my thermometers, the whole sky was constantly overcast with clouds. The atmosphere was, for the greater part of that time, very still, and a thermometer on the snow was generally about 2° lower, than another in the air. That this difference was not owing to evaporation was proved by the thermometer on the snow always rising, from a half to a whole degree, whenever the air was a little moved, and falling the same quantity, as soon as a great stillness again took place.

I had no opportunity of renewing my observations upon snow, before the beginning of the present year, 1814. The state of my health rendering it improper, that I should incur much fatigue, or be long exposed to night air, I restricted myself to the making a few experiments, in the large garden in Lincoln's-Inn Fields. I went thither, for the first time, on the evening of the 4th of January, immediately after a considerable snowfall had ceased, wishing to begin my observations, before any cold should arise on the snow's surface, from exposure to the sky. This was desirable on another account; for Mr. Kirwan, in direct opposition to indisputable facts, most clearly stated by Mr. Wilson, had said, that the great cold, observed by that gentleman on snow, was occasioned by this substance having retained the temperature of the high region, from which it had fallen.* The result of my enquiry was, that the

* On Temperatures, p. 30.

surface of the snow, and the air 4 feet above it, had precisely the same heat. The depth of the snow was 4 inches.

My next experiment took place on the evening of the 6th, the intervening day having been snowy. The sky was clear, but the air had a considerable motion. The heat of the atmosphere, at the height of 4 feet, was at $9\frac{1}{2}$h. 26°; while that of the surface of the snow, and of swandown lying upon it, was 22°. The depth of the snow was now about 5 inches.

On the 7th, a little after sunset, the heat of the air in the garden was 23°, that of the surface of snow 19°, but that of swandown lying upon the snow only 15°. There was then a gentle breeze; some parts of the sky were covered with clouds, and the lower atmosphere was a little obscure. While the exposed surface of the snow was 19°, a part of its surface, which had been covered, about 20 minutes, with a piece of pasteboard, was 22°. Grass, at the bottom of the snow, was 31°, and the earth an inch beneath the grass 32°.

After this, there was no fit time for observation until the 13th. The thermometers were exposed at 8 h. on the evening of that day, the sky being then without clouds; but the stars were not bright, and there was a perceptible motion in the air. At $8\frac{1}{2}$ h. the temperature of the air was $22\frac{1}{2}$°, that of the surface of the snow 13°, and that of swandown, lying on the snow, 8°. At 9 h. the air was $23\frac{1}{2}$°, snow 17°, and swandown 15°. The sky being now, in great measure, covered with high thin clouds, my experiments ceased. At $10\frac{1}{2}$ h. the sky was very bright, and the atmosphere very calm; but it was not then convenient to me to renew my observations. Had I repeated them at that time, I should probably have found a difference, between the temperature of the swandown and air, several degrees more considerable than the one of $14\frac{1}{2}$°, which had already occurred on this evening, and consequently greater than the greatest observed by Mr. Wilson, between the temperatures of snow and of the atmosphere, which was one of 16°.

The next favourable evening was that of the 21st. Much snow having in the meanwhile fallen, its depth was now more than a foot. The thermometers were observed 5 times between 4 h. 15 m. and 4 h. 55 m. At 4 of those times, the swandown was 13°, and at one of them $13\frac{1}{2}$°, colder than the air, the heat of which at the 4 first observations was 26°, and at the last $25\frac{1}{2}$°. The temperature of the surface of the snow, during the whole period of observation, was 17°, and consequently 4 times 4°, and once 5°, less cold, than that of the swandown. The atmosphere was altogether free from clouds, and nearly quite calm, but a good deal hazy.

Before another proper evening arrived, my health became so infirm, that I was obliged to relinquish this pursuit. I conclude therefore my account of it, with two remarks. 1. If Mr. Wilson had been accustomed to examine the temperature of swan-down, or any similar substance, placed upon snow, he would, probably, have observed a cold, on the surface of the earth, exceeding that of the atmosphere by 20° or more, on the night of his actually observing an excess of 16°. 2. Since upon one evening, when the atmosphere was neither very clear nor very still, a difference of 14½° was found by me, between the temperatures of air and of swandown, which is only ½ a degree less, than the greatest difference I have ever observed, between the same substances on the stillest and clearest nights in summer, a corroboration is hence derived of a conclusion, made by Mr. Six from his experiments, that the greatest differences at night, in point of temperature, between bodies on the surface of the earth, and the atmosphere near to it, are those which take place in very cold weather.

PART II.

OF THE THEORY OF DEW.

Dew, according to Aristotle,* is a species of rain, formed in the lower atmosphere, in consequence of its moisture being condensed by the cold of the night into minute drops. Opinions of this kind, respecting the cause of dew, are still entertained by many persons, among whom is the very ingenious Mr. Leslie of Edinburgh.† A fact, however, first taken notice of by Gersten, who published his treatise on dew in 1733, proves them to be erroneous; for he found, that bodies a little elevated in the air often become moist with dew, while similar bodies, lying on the ground, remain dry, though necessarily, from their position, as liable to be wetted, by whatever falls from the heavens, as the former.

Shortly after the appearance of Gersten's treatise, Musschenbroek made the remark, already mentioned in this Essay, that metals will be free from dew, while other bodies attract it copiously. This philosopher contented himself with publishing his

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* Meteor. Lib. 1. c. x. et De Mundo. c. iii.

† Relations of Heat and Moisture, p. 37, and 132.

discovery; but his friend Dufay concluded from it, that dew is an electric phænomenon, since it leaves untouched the bodies, which conduct electricity, while it appears upon those, which cannot transmit that influence. If dew, however, were to form on the latter only, its quantity would never be sufficiently great, to admit its being distinctly seen; for the non-conductors, as soon as they became in the least moist, would be changed into conductors. Charcoal, too, it is now known, though the best solid conductor of electricity after the metals, attracts dew very powerfully; and, in the last place, contrary to the assertion of Dufay, dew frequently forms upon metals themselves.

Other authors have ascribed the production of dew to electricity, for reasons different from that of Dufay. But there are several considerations, which seem to me to prove, that no such opinion can be just. 1. When dew is produced in a clear atmosphere, the portion of air, by which it is deposited, must necessarily be unable, at that moment, to retain, in a state of pellucid vapour, all the moisture, which it had immediately before held in that form. But I know of no experiment which shews, that air, by becoming positively electrical, which it is said to be its condition on the evenings, during which dew is most abundant, is rendered less able, than it had previously been, to contain watery vapour in a state of transparency. 2. Bodies, in similar circumstances, as far as electricity is concerned, acquire very different quantities of dew. Wool placed on the raised board, for example, attracted very much more dew, than wool attached to the lower side of the same board, and even considerably more than the same substance freely suspended in the air, and entirely exposed to the sky. 3. Dew forms in different parts of the night, in quantities no way proportioned to the degrees of electricity found in the atmosphere at the same times. Thus, it is commonly more copious, in the morning than in the evening, notwithstanding that the air is observed to be, in the latter season, more highly electrical than in the former. 4. I have several nights held a glass bottle, upon which dew was forming, close to the top of a Bennett's electrometer, which had been previously kept in a dry place; but I never saw the slips of gold leaf to move in consequence. It is very probable, however, that more refined experiments will shew, that electrical appearances attend the production of dew. These, perhaps, accompany every change in the chemical form of bodies. But the facts, which have been stated, seem sufficient to establish, that any such appearances, which may be hereafter remarked, during the formation of dew, must be considered as effects, and not as the cause, of the conversion of the watery vapour of a clear atmosphere into a fluid.

A remaining argument applies equally to all the theories, which have hitherto been made public on the cause of dew. This is, that none of them include the important fact, that its production is attended with cold; since no explanation of a natural appearance can be well founded, which has been built without the knowledge of one of its principal circumstances. It may seem strange to many, that neither Mr. Wilson, nor Mr. Six, applied this fact to the improvement of the theory of dew. But according to their view of the subject, no such use could have been made of it by them, as they held the formation of that fluid to be the cause of the cold observed with it. I had many years, as was formerly mentioned, held the same opinion; but I began to see reason, not long after my regular course of experiments commenced, to doubt its truth, as I found that bodies would sometimes become colder than the air, without being dewed; and that, when dew was formed, if different times were compared, its quantity, and the degree of cold which appeared with it, were very far from being always in the same proportion to each other. The frequent recurrence of such observations at length converted the doubt of the justness of my ancient opinion, into a conviction of its error, and at the same time occasioned me to conclude, that dew is the production of a preceding cold in the substances, upon which it appears. Wishing, however, to obtain proofs, more striking in degree, of the validity of these inferences, than such as had been afforded to me by casual observation, while attending to other parts of my subject, I instituted the experiments which will be next related.

I had frequently remarked, early in the evening, a considerable degree of cold on substances exposed in calm weather to a clear sky, and I had also sometimes seen, early in the evening, the raised board altogether dry, while the grass was much moistened. I therefore determined to make the experiments in view on the raised board, and to commence them as soon as the sun should cease to shine upon it. The first day I went to the country for this purpose, the 19th of August, 1813, almost every circumstance was favourable to its completion. There had been no rain for three weeks; the wind was northerly; and the barometer was rising; all which indicated, that the atmosphere contained little moisture. The air too was extremely still. The only appearance in the least unfavourable was, that the sky was not entirely free from clouds; but these were few, of small extent, thin, and high.

At 6 h. 25 m. immediately after the sun had ceased to shine upon the spot, where my experiments were to be carried on, though the time of its setting was still 47 minutes distant, I placed upon the raised board 10 grains of wool, and a small bag, made of the skin of a swan's breast with the down adhering, and stuffed with wool, the whole weighing nearly 5 drachms. On each of these substances the naked bulb of a small and delicate thermometer was laid. A similar thermometer, with its bulb also naked, was suspended in the air, over the grassplat, at the same height with the board. Two thermometers were placed in other situations, as will be seen in the annexed Table. After an exposure of 20 minutes, the wool was 7° colder than the air, but the swandown bag only 6°, no doubt in consequence of its comparatively great quantity of matter. Neither, however, had gained the least weight, according to the scales employed by me, which were sensibly moved by the 16th of a grain. These observations were repeated several times during the following hour, as will be seen by the Table, at none of which, except the last, was either the wool or swandown found in the least heavier, than when first placed on the board. At this last observation, the wool, though 9½° colder than the air, was still without any increase in weight; but the swandown, which was 1° colder than the wool, had gained ½ a grain. My experiments now properly ceased; but having suffered the thermometers, which had been placed on the wool and swandown, and in the air, to remain in those situations, I examined them again at 8 h. 45 m., that is, 2 h. 20 m. after they had been first exposed. The wool, which was still $9\frac{1}{2}°$ colder than the air, had gained somewhat less than $\frac{1}{2}$ a grain; and the swandown, which was now $11\frac{1}{2}°$ colder than the air, had gained 2 grains, including the $\frac{1}{2}$ grain already mentioned. When these last observations were made, the sky was entirely cloudless, and the atmosphere very calm.

TABULAR VIEW OF OBSERVATIONS

ON THE EVENING OF AUGUST 19, 1813.

	6h. 45m.	7h.	7h. 20m.	7h. 40m.	8h. 45m.
Heat of air 4 feet above the grass	$60\frac{1}{2}°$	$60\frac{1}{2}°$	59°	58°	54°
——— wool on the raised board	$53\frac{1}{2}$	$54\frac{1}{2}$	$51\frac{1}{2}$	$48\frac{1}{2}$	$44\frac{1}{2}$
——— swandown on the same	$54\frac{1}{2}$	53	51	$47\frac{1}{2}$	$42\frac{1}{2}$
——— surface of the raised board	58	57	$55\frac{1}{2}$		
——— grassplat*	53	51	$49\frac{1}{2}$	49	42

Similar experiments made at the same place, on the evenings of the 25th of August and 17th of September, in the same year, had results, which were also similar but less in degree; the greatest difference between the temperature of wool or swandown, while they were without any increase of

* In these experiments, contrary to what usually happens, the grass was almost constantly colder than the filamentous substances, although they were placed upon the raised board.

weight, and the temperature of the air, having been, on the first of those evenings, only 4°, and on the second only 5°. The reasons were, in great measure, if not wholly, that a considerable part of the sky was covered with clouds, and that the air was commonly in that state of motion, which is denominated a gentle breeze.

On the evening of my first experiments, I had omitted to measure the heat of the raised board, before the thermometers were placed upon it. This was attended to on the two latter evenings, on the first of which its upper surface was found, at the commencement of the experiments, 4° warmer than the air; on the second, both it and the air were of the same temperature. Again; on the first of the latter evenings, 10 grains of wool, to which 3 grains of water had been added, having been laid on the raised board, near the thermometers; at the end of 45 minutes the parcel was found to have lost 2½ grains of moisture by evaporation, during the time, that dry wool had become several degrees colder than the air.

A fourth experiment of this kind was made by me on the 7th of January, 1814, in the garden of Lincoln's-Inn Fields, by placing 10 grains of wool on a sheet of pasteboard, which lay upon the snow. At the end of 35 minutes the wool was 5° colder than the air, without possessing any additional weight.

Having thus shewn the justness of my former conclusion, that the cold, observed with dew, is the previous occurrence, and, consequently, that the formation of this fluid has precisely the same immediate cause, as the presence of moisture upon the outside of a glass or metallic vessel, when a liquid considerably colder than the air has been poured into it shortly before; I shall next apply this fact to the explanation of several atmospherical appearances.

I. The variety in the quantities of dew, which were found by me upon bodies of the same kind, exposed to the air during the same time of the night, but in different situations, is now seen to have been occasioned by the diversity of temperature, which existed among them.

II. Agreeably to the opinion of Mr. Wilson and Mr. Six, the cold connected with dew ought always to be proportional to the quantity of that fluid; but this is contradicted by experience. On the other hand, if it be granted, that dew is water precipitated from the atmosphere, by the cold of the body on which it appears, the same degree of cold, in the precipitating body, may be attended with much, with little, or with no dew, according to the existing state of the air in regard to moisture; all of which circumstances are found actually to take place.

III. The formation of dew, indeed, not only does not produce cold, but, like every other precipitation of water from the atmosphere, produces heat. I infer this, partly because very little dew appeared upon the two nights of the greatest cold I have ever observed on the surface of the earth, relatively to the temperature of the air, both of them having occurred after a long tract of dry weather; and partly from the most dewy night, which I have ever seen, having been attended, during the greater part of it, with no considerable degree of cold. On this night, the difference between the temperatures of grass and of air was at first 7½°, the dew being then not very abundant. But, after the dew had become very abundant, the difference of those temperatures never exceeded 4°, and was frequently only 3°.

With the view of obtaining, though indirectly, some knowledge of the quantity of cold, which had been prevented, by the formation of dew, from appearing on the surface of the earth, in the night just spoken of, I made the following experiment. To 10 grains of wool having the same form and extension, as the parcels employed for the collection of that fluid, were added 21 grains of water, this being the quantity of moisture, which had been attracted by 10 grains of wool, lying on the grassplat, in the space of 8 hours on that night. The wet wool having been then placed in a china saucer, laid on a feather-bed in a room, the door and windows of which were shut, its heat during the following 8 hours was, at frequent examinations, uniformly found to be about 4° less, than that of a dry china saucer on the same bed; the temperature of the air in the room not having altered more than $\frac{1}{2}$ a degree, in the course of the experiment. At the end of the 8 hours, the wool still retained $2\frac{1}{2}$ grains of moisture. If this quantity had also evaporated, the cold uniformly produced during the 8 hours would, in all probability, have been about $4\frac{1}{2}°$. From this experiment, therefore, I think it may be inferred, that the mean quantity of cold, which was prevented, by the formation of dew, from appearing on the ground, during the night which has been mentioned, was also about $4\frac{1}{2}°$. But, as the production of dew, during some parts of the night, was at a greater rate, than that of 21 grains for 8 hours, 1 or 2 degrees may be added for those times, which will raise the effect of the dew in diminishing the appearance of cold during them to about 6°, on the supposition, which cannot be far from the truth, that dew had been attracted as copiously by the grass, as by wool which lay upon it.

The less difference commonly observed between the temperatures of grass and of air, in the morning, than what occurs in the evening, is likewise to be, in part, attributed to a greater quantity of dew appearing in the former, than in the latter season.

A more remarkable fact, deriving an explanation from the same source, is the greater difference which takes place in very cold weather, if it be calm and clear, between the temperatures of the air and of bodies on the earth, at night, than in equally clear and calm weather in summer; since, in very cold weather, any diminution of the temperature of a portion of air, in contact with a cold body, will be attended, in consequence of the well known relations of the atmosphere to moisture, with a much less formation of water, than an equal diminution would be in summer, supposing the air, before it touches the cold body, to be at both times equally near to its point of repletion with moisture.

IV. In very calm nights, a portion of air, which comes in contact with cold grass, will not, when the surface is level, immediately quit it, more especially, as this air has become specifically heavier than the higher, from a diminution of its heat, but will proceed horizontally, and be applied successively to different parts of the same surface. The air, therefore, which makes this progress, must at length have no moisture to be precipitated, unless the cold of the grass which it touches should increase. Hence in great measure is to be explained, why on such nights, as have been just mentioned, more dew was acquired by substances placed on the raised board, than by others of the same kind on the grass, though it began to form much sooner in the latter than in the former situation, those on the raised board having received air, which had previously deposited less of its moisture.

A reason is now also afforded, why a slight agitation of the atmosphere, when very pregnant with moisture, should increase the quantity of dew; since fresh parcels of air will hence be more frequently brought into contact with the cold surface of the earth, than if the atmosphere were entirely calm.

V. Dew, in agreement with the immediate cause which has been assigned by me for its production, can never be formed, in temperate climates, upon the naked parts of a living and healthy human body, during the night; since their heat is never less in this season, in such climates, than that of the atmosphere. I have, in fact, never perceived dew on any naked part of my own body at night, though my attention was much occupied, for three years, with every thing relative to this fluid, and though I had been, during that period, much exposed to the night air. On the other hand, in very hot countries, the uncovered parts of a human body may sometimes, from being considerably colder than the air, condense the watery vapour of the atmosphere, and hence be covered with a real dew, even in the day time.

VI. Hygrometers formed of animal or vegetable substances, when exposed to a clear sky at night, will become colder than the atmosphere; and hence, by attracting dew, or, according to an observation of Saussure,* by merely cooling the air contiguous to them, mark a degree of moisture, beyond what the atmosphere actually contains. This serves to explain an observation made by Mr. De Luc,† that in serene and calm weather, the humidity of the air, as determined by an hygrometer, increases about, and after sunset, with a greater rapidity, than can be attributed to a diminution of the general heat of the atmosphere.

These examples are sufficient to shew the value of the fact, that bodies become colder than the neighbouring air, before they are dewed, in explaining many atmospherical appearances. To this point, the investigation of the cause of dew might have been carried at any time, since the invention of thermometers; but its complete theory could not possibly, in my opinion, have been attained, before the discoveries on heat were made, which are contained in the works of Mr. Leslie and Count Rumford.

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* Hygronometrie, p. 25.

† Introduction a la Physique Terrestre, II. 491.

The experience of most persons, respecting the communication of heat among bodies in the open air, is confined to what happens during the day; at which time, those that are situated near to one another are always found to possess the same temperature, unless some very evident reason for the contrary should exist. To many, therefore, it may appear incredible, that a perfectly dry body, placed in contact, on all sides, with other bodies of the same temperature with itself, shall afterwards, without undergoing any chemical change, become much colder than they are, and shall remain so for many hours; yet these circumstances are found to occur in substances attractive of dew, when laid on the surface of the earth, in a still and serene night, and are in perfect agreement with the doctrine of heat, now universally admitted to be just.

To render this more easy of apprehension, let a small body which radiates heat freely, and possesses a temperature, in common with the atmosphere, higher than 32°, be placed, while the air is clear and still, on a slow conductor of heat lying on the surface of a large open plain, and let a firmament of ice be supposed to exist at any height in the atmosphere; the consequence must be, that the small body will, from its situation, quickly become colder than the neighbouring air. For, while it radiates its own heat upwards, it cannot receive a sufficient quantity from the ice to compensate this loss; little also can be conveyed to it from the earth, as a bad conductor is interposed between them; and there is no solid, or fluid except the air, to communicate it laterally either by radiation or conduction. This small body, therefore, unless it shall receive from the air, nearly as much heat as it has emitted, which, considering the little that can be communicated from one part of the atmosphere to another, in its present calm state, must be regarded as impossible, will become colder than the air, and condense the watery vapour of the contiguous parts of it, if they should contain a sufficient quantity to admit of this effect. But events similar to these occur, when dew appears in an open and level grass field, during a still and serene night. The upper parts of the grass radiate their heat into regions of empty space, which consequently send back no heat in return; its lower parts, from the smallness of their conducting power, transmit little of the earth's heat to the upper parts, which at the same time receiving only a small quantity from the atmosphere, and none from any other lateral body, must remain colder than the air, and condense into dew its watery vapour, if this be sufficiently abundant, in respect to the decreased temperature of the grass.*

* I have adopted in this explanation the hypothesis of Mr.

This subject may be further illustrated by a reference to what happens in the experiment, which has been used to prove the reflection of cold.

In the simplest form of this experiment, a small body, the bulb of a thermometer, possessing the temperature of the atmosphere, is placed before a larger cold body, rendered equal in effect to one still larger, by means of a concave metallic mirror. In this situation, the small body radiates heat to the larger, without receiving an equivalent from it, and, in consequence, becomes colder than the air through which its heat is sent, notwithstanding that it is continually gaining some heat, both from the air which surrounds it, and from the walls and contents of the apartment, in which the experiment is made. Dew, therefore, would as readily form upon the thermometer in this experiment, as it would upon one suspended in the open air at night, under a clear sky, provided that the two instruments were equally colder than the atmosphere, and

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Prevost of Geneva, on the constant radiation of heat by bodies in contact with the atmosphere, even at the time that they are exposed to the influence of bodies warmer than themselves; as it appears to agree perfectly with all the phænomena of the communication of heat, which do not depend upon conduction. I shall hereafter make frequent use of this hypothesis.

that this was in both cases equally near to being replete with moisture.*

Regarding now as established, that bodies situated on or near to the surface of the earth become, under certain circumstances, colder than the neighbouring air, by radiating more heat to the heavens, than they receive in every way, † I shall in the

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* The invention of this experiment having been ascribed a few years ago to Mr. Pictet of Geneva, various English writers have shewn, that it occurs in several much older foreign authors. But I have not seen any mention made of its having been also long since known in this country. That it was so appears from the following extract of a letter, written by Mr. Oldenburgh to Mr. Boyle in 1665. 'I met the other day in the Astrological Discourse of Sir Christopher Heydon, with an experiment, which he affirms to have tried himself, importing, that cold accompanies reflected light, by employing burning spherical concaves, or parabolical sections, which, he saith, will as sensibly reflect the actual cold of snow or ice, as they will the heat of the sun.' *Boyle's Works*, folio, vol. V. p. 345.

† Count Rumford offered the following conjecture, in a paper printed in the Philosophical Transactions for 1804. 'The excessive cold which is known to reign, in all seasons, on the tops of very high mountains, and in the higher regions of the atmosphere, and the frosts at night, which so frequently take place on the surface of the plains below, in very clear and still weather, in spring and autumn, seem to indicate, that frigorific rays arrive continually at the surface of the earth, from every part of the heavens.' But he gave no experiments to prove, that such a communication actually exists between the heavens and the earth at night. Neither does it appear from any of his writings which

first place offer a few remarks on the extent and use of this occurrence, and shall afterwards apply the knowledge of it to the explanation of several more of the appearances described in the former part of this Essay, and of some others, which have not hitherto been mentioned by me.

Radiation of heat by the earth to the heavens must exist at all times; but, if the sun be at some height above the horizon, the degree of which is hitherto undetermined, and probably varies according to season, and several other circumstances, the heat emitted by it to the earth will overbalance, even in places shaded from its direct beams, that which the earth radiates upwards. I suspended at midday, on the 24th of July, 1813, in the open air over a grassplat, while the sky was wholly covered with very dense clouds, and the weather calm, two delicate thermometers, one of which was naked, but the other cased in gold paper. At two observations, having an interval of 10 minutes between them, the thermometer in the gilt case was 2° lower than that which was naked. A white paper case was then drawn over the gilt one, upon which, after 5

I have seen, that he ever supposed, that the surface of the earth is more cooled by these frigorific rays, than the air through which they pass, or that some solid bodies are more cooled by them than others.

minutes, the covered instrument was observed to be at the same height with the naked. The outer white case having, in the next place, been taken from the covered thermometer, but that which was gilt suffered to remain, the two instruments were in a few minutes found again to differ 2°. A thermometer on the grassplat was, during these experiments, higher than the naked instrument in the air by 2°, and than that in the gilt case by 4°. It is evident, therefore, that heat radiated by the sun must, on this day, have been transmitted in considerable quantity through the thickest clouds; since not only was the earth's surface warmer than the air, but a small body, covered with a substance not readily admitting the entrance of radiant heat, was colder than a similar body which was uncovered. In like manner, I observed at noon, on the 2nd of January, 1814, during the prevalence of a dense fog, a thermometer placed upon swandown, which was lying upon grass thickly incrusted with hoarfrost, to be 2° warmer than the air, and 1° warmer than the grass.*

In a calm and serene night, however, when

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* Another fact of the same kind, which occurred at the same time, is that, although the temperature of the air was 30°, the hoarfrost on trees rapidly decreased, the solid matter of the trees intercepting radiant heat, which had penetrated through the fog from the sun, and converting it into heat of temperature.

consequently little impediment exists to the escape, by radiation, of the earth's heat to the heavens, and when no heat can be radiated by the sun to the place of observation, an immense degree of cold would occur on the ground, if the following circumstances did not combine to lessen it. 1. The incapacity of all bodies to prevent, entirely, the passing of heat, by conduction, from the earth to substances placed upon them. 2. The heat radiated to these substances by lateral objects. 3. The heat communicated to the same substances by the air. 4. The heat which is evolved, during the condensation of the watery vapour of the atmosphere into dew.

The extent of the effect of all these checks upon the production of cold, by the nightly radiation of heat from bodies on the surface of the earth, cannot, in the present state of our knowledge, be properly estimated; but facts shew that, notwithstanding their operation, the cold originating in this source must be often very considerable.

1. Mr. Wilson once observed a difference of 16°, from this cause, between the temperatures of snow and of air. In taking the latter temperature, however, he employed a naked thermometer, on which account, in consequence of what has already been mentioned by me, about 2° are to be added to the 16° noted by him, in order to obtain the

real difference between the heat of the snow and the air at that time.*

2. If Mr. Wilson, as was formerly said, had laid a thermometer on any downy substance in contact with the snow, he would, in all probability, have found a cold indicated by it at least 20° greater than that of the air, as marked by a naked instrument, and consequently at least 22° greater than the real cold of the surrounding atmosphere.

3. Mr. Wilson's place of observation was not very favourable to the occurrence of a great cold, from radiation of heat at night, it being near to a large smokey city, in the immediate vicinity also, as appears to me from what he says of it, of one or more considerable buildings, and in a climate abounding in moisture.

4. None of Mr. Wilson's experiments, in which a very great degree of cold occurred, were made within an hour or two after sunset, during which time, according to my observation, the most considerable differences between the temperatures of

* As bright metals, when suspended in the air, and exposed to a clear sky on a calm night, become colder than the surrounding atmosphere, a thermometer covered with metalled paper, and placed in the circumstances which have been just mentioned, will mark a temperature less than that of the air near to it. But, as the difference must be small, and as I know of no way to estimate it accurately, I have hitherto always neglected to consider it.

the air, and of bodies on the surface of the earth, commonly happen.

If, then, such experiments should be made in an atmosphere still colder than that, in which Mr. Wilson made his, on a large plain remote from any city, and free from objects of every kind, that are elevated above the ground, and in a country remarkable for the dryness of its air, all which circumstances may be found in Russia during the winter; a difference of at least 30° would probably appear, on some still and serene night, between a small thermometer placed with its bulb naked,* on the middle, or leeward side of a stratum of a downy substance, occupying a space upon a grass field, or bed of snow, one or two square yards in extent, and a similar thermometer inclosed in a case of gilt paper, and suspended in the air a few feet above the other. Two thermometers, thus placed, would, I think, be sometimes found even in this country to differ not much less than 30°. I have myself never made any such experiments with a downy substance, which had a surface of more than a few square inches, or in a very cold night, when the atmosphere was clear and calm, and the scene of observation remote from large masses of building.

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* The effect would, perhaps, be a little increased, by covering the bulb with a very thin layer of lamp-black.

But even a cold of 30° appears not to be the greatest, that can be thought to occur, from the radiation of heat to the heavens, at night, by substances on the surface of the earth. For experiments by Mr. Pictet,* Mr. Six,† and I may add by myself, establish that, in exception to the common rule, the heat of the atmosphere in clear and calm nights *increases* with the distance from the earth. Agreeably to Mr. Six's experiments, the atmosphere at the height of 220 feet is often, upon such nights, 10° warmer than what it is 7 feet above the ground. If, therefore, I am able to shew, as I expect I shall be in the course of a few pages, that the air at the smaller height becomes colder than that of the greater, from its vicinity to the surface of the earth, previously rendered cold by radiating its heat to the heavens, it will follow, that these 10° must be added to the quantity of cold already mentioned; and, consequently, that a body on the ground may become, at night, at least 40° colder than the air two or three hundred feet above it, by the radiation of its heat to a clear sky.

I shall add, with the greatest diffidence, a few words upon a final cause of the radiation of heat from the earth at night, and upon some of the circumstances which modify its action, though fully conscious of the danger of error, which is always

* Essai sur le Feu, c. x.

† Phil. Trans. 1784, and 1788.

incurred in the attempt to appreciate the works of our Creator.

The heat which is radiated by the sun to the earth, if suffered to accumulate, would quickly destroy the present constitution of our globe.* This evil is prevented by the radiation of heat by the earth to the heavens, during the night, when it receives from them little or no heat in return. But, through the wise economy of means, which is witnessed in all the operations of Nature, the prevention of this evil is made the source of great positive good. For the surface of the earth, having thus become colder than the neighbouring air, condenses a part of the watery vapour of the atmosphere into dew, the utility of which is too manifet to require my speaking of it. I may remark, however, that this fluid appears chiefly where it is most wanted, on herbage, and low plants, avoiding, in great measure, rocks, bare earth and considerable masses of water.† Its production too, by an-

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* Count Rumford says; 'May it not be by the action of these [frigorific] rays, that our planet is cooled continually, and enabled to preserve the same mean temperature for ages, notwithstanding the immense quantities of heat that are generated at its surface, by the continual action of the solar rays?' Phil. Trans. 1804, p. 181.

† I have no direct observations for the foundation of this assertion concerning considerable masses of water. But, I hold it, notwithstanding, to be just; because, as soon as the surface of the water is in the least cooled by radiation, the particles com-

other wise arrangement, tends to prevent the injury, that might arise from its own cause; since the precipitation of water, upon the tender parts of plants, must lessen the cold in them, which occasions it. I shall observe in the last place, that the appearance of dew is not confined to any one part of the night, but occurs during its whole course, from means the most simple and efficacious. For after one part of the air has deposited its moisture, on the colder surface of the earth, it is removed, in consequence of that agitation in the atmosphere which exists during its stillest states, and gives place to another having its quantity of water undiminished; and, again, as the night proceeds, a portion of air, which had before deposited all the moisture, which circumstances at that time permitted, is rendered fit, by the general increase of the cold of the atmosphere, to give out a fresh parcel, when it comes anew into contact with the ground.

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posing it must fall downwards, from their increased gravity, and be replaced by others that are warmer. The whole mass, therefore, can never, in the course of a single night, be sufficiently cooled to condense into dew any great quantity of the watery vapour of the atmosphere. Besides; I have found, that even a small mass of water, as will be more particularly mentioned in the last part of this Essay, sometimes acquires no weight from the reception of dew, in the space of a whole night favourable to the formation of that fluid.

I. The first fact, which I shall here attempt to explain, is the prevention, either wholly or in part, of cold, from radiation, in substances on the ground, by the interposition of any solid body between them and the sky. This evidently appears to arise in the following manner. The lower body radiates its heat upwards, as if no other intervened between it and the sky; but the loss, which it hence suffers, is more or less compensated by what is radiated to it, from the body above, the under-surface of which possesses always the same, or very nearly the same temperature as the air. In this way therefore, is to be accounted for the warmth of the substances, which were sheltered from the sky by the raised board, the pasteboard roof, and the hollow cylinders of earth and pasteboard. In these examples, the interposed substances cannot be supposed to have remitted more heat than they received. But in situations where large masses of bare solid matter exist, which are warmer than the atmosphere, from the heat of the preceding day or other causes, a greater heat will be received by the exposed body, than what is radiated by itself. For example, it seems certain to me, that the houses, surrounding Lincoln's-Inn Fields, had an influence upon my thermometers, during my experiments there at night, beyond what arose from their merely returning a quantity of heat, equivalent to that, which they received from the surface of the garden. It is not, however, absolutely requisite, that a body should be itself exposed to the sky on a clear and calm night, in order to become colder than the atmosphere; exposure to the influence of another body, so situated, is sufficient for the production of a slight degree of this effect. Thus, I have always found wool attached to the underside of my raised board, on such a night, to be a little colder than the air; and it has appeared to me a sufficient reason for the fact, that the wool in this situation was, in some degree, exposed to the influence of grass, which had become considerably colder than the atmosphere, by radiating its heat to the sky.

II. No direct experiments can be made to ascertain the manner, in which clouds prevent, or occasion to be small, the appearance of a cold at night, upon the surface of the earth, greater than that of the atmosphere; but it may, I think, be firmly concluded, from what has been said in the preceding article, that they produce this effect, almost entirely, by radiating heat to the earth, in return for that which they intercept in its progress from the earth towards the heavens. For although, upon the sky becoming suddenly cloudy during a calm night, a naked thermometer, suspended in the air, commonly rises 2 or 3 degrees, little of this rise is to be attributed to the heat evolved by the condensation of watery vapour in the atmosphere, as was supposed by Mr. Wilson;* since,

* Edin. Phil. Trans. I. 157.

in consequence of the ceasing of that part of the cold indicated by the thermometer, which was owing to its own radiation to a clear sky, the temperature of the atmosphere may seem to increase 2°, or more, notwithstanding that it has received no real addition. Besides; the heat which is extricated by the condensation of vapour, during the formation of a cloud, must soon be dissipated; whereas the effect of greatly lessening, or preventing altogether, the appearance of a superior cold on the earth to that of the air, will be produced by a cloudy sky, during the whole of a long night.

Dense clouds, near the earth, must possess the same heat as the lower atmosphere, and will therefore send to the earth, as much, or nearly as much heat as they receive from it by radiation. But similarly dense clouds, if very high, though they equally intercept the communication of the earth with the sky, yet being, from their elevated situation, colder than the earth, will radiate to it less heat than they receive from it, and may, consequently, admit of bodies on its surface becoming several degrees colder than the air. In the first part of this Essay, an example was given of a body on the ground becoming at night 5° colder than the air, though the whole sky was thickly covered with high clouds.*

* Mr. Prevost of Geneva, in his work on Radiant Heat, p. 382,

Islands, and parts of continents close to the sea, being, by their situation, subject to a cloudy sky, will, from the smaller quantity of heat lost by them through radiation to the heavens at night, in addition to the reasons commonly assigned, be less cold in winter, than countries considerably distant from any ocean.

III. Fogs, like clouds, will arrest heat, which is radiated upwards by the earth, and, if they be very dense, and of considerable perpendicular

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has already in this way, conjecturally, accounted for the effect of clouds, in diminishing, at night, the cold of the atmosphere, and of the surface of the earth; but he seems not to have known, that their effect on the temperature of the latter is much greater than that which they produce upon the air. My explanation of this influence of clouds, on the temperature of the surface of the earth, during the night, is a direct consequence from the facts, which I had observed respecting the prevention of cold on the ground from radiation, by the interposition of solid bodies between it and the heavens, and occurred to me in 1812. Mr. Prevost's work, indeed, was published in 1809; but I did not see it before the summer of 1813; when it was lent to me by his relation Dr. Marcet of London, who at the same time said, that he believed there was no other copy of it in Great Britain, except one, which had been sent by himself to Edinburgh.

Note to second edition.] I did not know, until after the first edition of this Essay was printed, that Mr. Prevost had published his opinion on the effect of clouds in preventing the occurrence of cold at night in the atmosphere, and upon the surface of the earth, as early as 1792, in a work entitled 'Recherches sur la Chaleur.'

extent, may remit to it as much as they receive. Accordingly, Mr. Wilson found no difference at night, in very foggy weather, between the temperature of the surface of snow, and that of the air. Several observations by myself tend to confirm that of Mr. Wilson. An instance, however, as was formerly said, occurred to me of a difference at night of 9° between the temperatures of grass crusted over with hoarfrost, and of air, during a very dense fog. A fact, remarked by Mr. Leslie, respecting fogs, serves to explain this apparent anomaly. For it was found by that philosopher,* from experiments made with his photometer, that in mists and low fogs the diminution of the sun's heat is small, when compared with what occurs, when the sky is obscured by a dense body of clouds; and it will, I presume, be readily granted, that the same state of the atmosphere, which allows the heat of the sun to pass copiously, will also give a ready transit to heat radiated by the earth. Now there are several reasons for believing, that the fog, during which grass was 9° colder than the air, did not ascend far above the ground. 1. The barometer had been falling for some days before, and it is a matter of common observation, that great fogs seldom occur, except it be high. 2. On the day preceding the observation, the air, after having

* On Heat and Moisture, p. 57.

been extremely foggy for nearly a week, had become clear enough to allow the sun's being distinctly seen during the whole of the afternoon, though there was still a sufficient obscurity in the lowmost parts of the atmosphere, to obstruct considerably the view of objects on the ground and very near to it. 3. On the day following the observation, the fog was again much less; on the next it disappeared, and was succeeded by snow. It is to be mentioned likewise, that on the evening in question the state of the grass, which was the subject of experiment, was unusually favourable to the production of cold; since, contrary to general experience, it was as cold as swandown. If, then, the latter substance, from the much greater regularity of the appearances exhibited by it, be taken as the standard, by which the occurrences of different nights are to be compared together, it will follow, that the fog of which I am speaking, though it did not prevent, must have lessened, the production of cold from radiation. For, on the preceding evening, when there was little fog, the atmosphere being equally still on both, the difference between swandown and the air was 12°; and on another, a fortnight after, the difference at the same place of observation, between thermometers in the same situations, was $14\frac{1}{2}$°, the air being now free from fog. If the atmosphere had been as still on this, as on the former evenings, a greater difference would doubtless have been seen. I conclude, therefore, that fogs do not in any instance furnish a real exception to the general rule, that whatever exists in the atmosphere, capable of stopping or impeding the passage of radiant heat, will prevent or lessen the appearance at night of a cold on the surface of the earth, greater than that of the neighbouring air.

It follows also, from what has been said in this article, that the water deposited upon the earth, during a fog at night, may sometimes be derived from two different sources, one of which is a precipitation of moisture from a considerable part of the atmosphere, in consequence of its general cold; the other, a real formation of dew, from the condensation, by means of the superficial cold of the ground, of the moisture of that portion of the air, which comes in contact with it. In such a state of things, all bodies will become moist, but those especially, which most readily attract dew in clear weather.* I have had no opportunity, however, of trying this conclusion by the test of observation, since it occurred to me.

IV. When bodies become cold from radiation, the degree of effect observed must depend, not only

* The moisture observed at night by Musschenbroek in Holland, and called by him dew, appears to me to have been of this kind. See p. 4, of this Essay.

on their radiating power, but in part also on the greater or less ease, with which they can derive heat, by conduction, from warmer substances in contact with them. Thus grass, on a clear and still night, was constantly colder, sometimes very much colder, than the gravel walk, though a small quantity of sand, placed upon grass, was always nearly as cold as this substance. In this case, the difference in temperature, between the gravel walk and sand, evidently depended on the different quantities of heat, which they received from the parts beneath. A like reason is to be given for dew appearing in greater quantity on shavings of wood, than on the same substance in a more dense and compact form; and for filamentous and downy substances becoming colder than all others, even than lampblack, which is placed by Mr. Leslie, at the head of the best solid radiators of heat. For the lampblack exposed by me, being about 2 lines in depth, possessed, in consequence, a fund of internal heat, which would more readily pass to its cold surface, than the heat of the lower parts of the downy substances would to their upper surface.

This subject is illustrated by the following experiment. On a dewy evening, I depressed into soft garden mould a drinking glass, having a thick flat bottom, until its brim was upon a level with the surrounding earth, and at the same time placed a similar vessel, with its cavity also towards the sky, on the surface of the mould. In the morning, the inside of the depressed glass was entirely dry, while that of the other was dewed. I then applied the bulb of a small thermometer to the inside of the bottom of each vessel, on which I found the heat of that part of the depressed one to be 56°, but of the same part of that which stood on the mould only 49½°. At this time the temperature of the air was 53°. The cause, therefore, was evident, both of the wetness of the first vessel, and of the dryness of the second.

From this source also is be derived the reason, why the prominent parts of various bodies were observed by Mr. Wilson to be crusted with hoar-froast, while their more retired and massy parts were free from it.*

V. Bodies, exposed in a clear night to the sky, must radiate as much heat to it during the prevalence of wind, as they would do if the air were altogether still. But in the former case, little or no cold will be observed upon them above that of the atmosphere, as the frequent application of warm air must quickly return a heat equal, or nearly so, to that which they had lost by radiation. A slight agitation of the air is sufficient to produce some effect of this kind; though, as has already been said, such an agitation, when the air is very preg-

* Paper in Phil. Trans. 1780.

nant with moisture, will render greater the quantity of dew, one requisite for a considerable production of this fluid being more increased by it, than another is diminished.

VI. A small body, as a thermometer, suspended in the air, will even in the calmest night exhibit but little cold from radiation, since it is continually exposed to the application of fresh parcels of warmer air, both from the progressive motion of this fluid, and from the downward motion produced in it by the superior gravity of such portions, as have been cooled by contact with the suspended body. On the other hand, a thermometer upon a board, raised above the earth and possessing a surface of several square yards, will have its cold from radiation much less diminished than the former, as it is exposed to no loss from a downward motion of the air, and as the air, which approaches it horizontally must, almost always, have had its temperature previously lowered, by passing over another part of the board. The reason then of the lee side of the raised board being often colder than the windward is obvious.

VII. There is a remark by Theophrastus,* which has been confirmed by other writers, that the hurtful effects of cold occur chiefly in hollow places. If this be restricted to what happens on

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* Lib. v. c. xvi.

serene and calm nights, and it does not, I believe, hold true in any other circumstances, two reasons from different sources are to be assigned for it. The first is, that the air being stiller in such a situation, than in any other, the cold, from radiation, in the bodies which it contains, will be less diminished by renewed applications of warmer air; the second, that from the longer continuance of the same air in contact with the ground, in depressed places than in others, less dew will be deposited, and therefore less heat extricated during its formation. It will be seen in the last part of this Essay, that, in the East Indies, depressions in the earth are artificially made, for the purpose of increasing the cold, which appears in serene nights. On this subject, however, it is to be observed, that if the depressed or hollow places be deep, in proportion to their horizontal extent, a contrary effect must follow; as a case will occur more or less similar to that which existed in some experiments formerly related by me, in which a small portion of grass was surrounded by a hollow cylinder.

VIII. An observation closely connected with the preceding, namely that, in clear and still nights, frosts are less severe upon hills, than in neighbouring plains,* has excited more attention, chiefly from

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* Theophrastus also remarks, that it freezes less on hills than on plains, but without mentioning, that this happens only on calm and serene nights. Lib. v. c. xx.

its contradicting what is commonly regarded an established fact, that the cold of the atmosphere always increases with the distance from the earth. This inferior cold of hills is evidently a circumstance of the same kind, with that ascertained by Mr. Pictet and Mr. Six, respecting the increasing warmth, in clear and calm nights at all seasons of the year, of the different strata of the atmosphere, in proportion as these are more elevated above the earth. As the greater cold of the lower air is the less complicated fact, I shall attempt to explain it in the first place. Mr. Pictet, indeed, furnishes an explanation himself, by ascribing it to the evaporation of moisture from the ground. But to shew that this is not just, it need only be mentioned, that the appearance never occurs in any considerable degree, except upon such nights as are attended with some dew, and that its great degrees are commonly attended with a copious formation of that fluid; since it cannot be thought, that the same stratum of air will deposit moisture on the ground, from an insufficiency of heat, at the very time it is receiving moisture from the ground, in the state of pellucid vapour, as this presupposes, that it is not yet replete with water.

Our atmosphere has been very generally regarded, as incapable of being heated directly by the rays of the sun, principally because these give no heat to any particular portion of it, in which they are brought to a focus. I do not know, whether this experiment was ever made with all the accuracy of which it is susceptible; but, granting that it has been thus made, my opinion is, notwithstanding, that no reliance can be placed in it, For as air, if heated at all by concentrated sunbeams, must be heated by them in a very slight degree, during the time that their focus may be looked upon as stationary, otherwise the present question would not have arisen, it is necessary for conducting the experiment properly, that, during the whole of it, the same individual small portion of air shall constantly receive that focus; but this, for various manifest reasons, cannot possibly happen. Viewing, therefore, the argument founded upon this experiment as without force, I shall now offer several considerations, which seem to prove, that air is actually heated by the sunbeams, which enter it.

1. Air both reflects and refracts light, and all other bodies, as far as I know, acquire heat, while they act thus on the light of the sun.

2. Air suffocates or absorbs the sun's light, which it cannot be supposed to do, without increasing its temperature.

3. If air, considered as an uniform fluid, were even incapable of gaining heat directly from the sun's rays, heat would be communicated by them

to it, through the intervention of the innumerable particles of solid matter, which the trivial experiment of receiving a sunbeam into a darkened room shews to be present in the atmosphere. Should it be said, that this appearance may occur only in the neighbourhood of the earth, from the accidental admixture of solid matter raised from its surface by winds, or in any other way, the answer is, that, as my inquiry is concerning the existence of a certain condition of the atmosphere, it matters not how this originates. Nothing more can be demanded, than that it should always be found, which I believe to be the case; since, if I can trust my memory with respect to what took place many years ago, I should say, that such particles are to be seen, by means of the sun's light, in the air over the middle of the Atlantic ocean. These particles then must receive heat from the sunbeams, which impinge upon them, and this they will communicate to the contiguous pellucid air.

4. Unless it be admitted, that the atmosphere is capable of intercepting part of the heat, which is radiated into it by the sun, and of converting this into heat of temperature, I deem it impossible to find a sufficient reason, for the great warmth which exists, after a long calm, in air incumbent upon the Atlantic and Pacific oceans, at the distance of a thousand miles or more from any

considerable body of land. It cannot be derived from the neighbouring water, since this is colder than the lower atmosphere; and no one will suppose it to be the same heat, which the air had acquired from the last continent it had passed over, many days before. But, if even this were supposed, another difficulty would remain to be removed, which is, that, during the whole of the calm, the air is cooled every night, and again becomes warm in the day.*

Should what has been said be thought sufficient to establish, that the air arrests part of the sun's heat, which is radiated into it bound up with light, two consequences must also be allowed. The first is, that air will exert a greater power of the same kind upon heat radiated into it without light, since the sun's heat passes instantaneously through many bodies, which refuse a similar way to heat radiated by terrestrial substances; the other, that air must be as capable of becoming cold by radiating its own heat,† as of becoming warm from heat radiated into it, as these two

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* One reason is hence apparent for the great coldness of the high regions of the atmosphere; since the air in them must be less fit, than that of the lower strata, to arrest heat which is radiated into it.

† Mr. Prevost says: 'On pent supposer que les molecules de l'air rayonnent.' Du Calorique Rayonnant, p. 24.

properties are uniformly observed to exist together, and to be proportional to each other. The truth of the latter conclusion may also be inferred from this fact, that in still and calm weather the heat of the air, a few feet above the earth, will sometimes decrease, even in this country, 18 or 20 degrees between sunset and sunrise, though no change of wind has in the meantime occurred; for the inconsiderable conducting power, which air is now known to possess, will permit only a small part of this diminution to arise from heat passing, by means of that power, from the atmosphere to the colder earth. Mr. Leslie,* indeed, ascribes this effect to the descent of cold air from the higher regions of the atmosphere; but if this were just, a less cold ought to be found, on a clear and still night, in the lower than in the higher strata, which is contrary to the uniform results of numerous experiments by Mr. Pictet and Mr. Six. Winds too, which produce such a mixture, always lessen the nocturnal decrease of temperature in the lowermost part of the atmosphere.

Having thus shewn, that air is capable, both of absorbing heat, which is radiated into it, and of radiating heat, which had before formed a part of its temperature, I proceed to apply the know-

* On Heat and Moisture, p. 11, and 132.

ledge of these facts, to the explanation of the phænomenon observed by Mr. Pictet and Mr. Six.

This phænomenon occurs on those nights only, which permit bodies, on the surface of the earth, to become cold by radiating their heat to the heavens. On other nights, when bodies, thus situated, were not colder than the air, I have observed the atmosphere, within the limits of 9 feet from the ground, the boundary of my own experiments, to decrease a little in temperature, as the distance from the earth increased. Mr. Six likewise found, that, on cloudy nights, the air was sometimes colder 220 feet above the ground, than at the distance of 9 feet from it. When, therefore, the earth has become colder, from radiation, than the neighbouring air, in consequence of the latter having, by reason of its small radiating power, emitted a less proportion of its heat to the heavens, the warmer air must radiate a part of its heat to the earth, without receiving a full compensation, and will therefore become colder, than it otherwise would have been. In proportion too as the air is nearer to the earth, must the cold of the former from this cause be the greater. My own conception of this matter is facilitated,* by contemplating the occurrence of an opposite effect, when the earth is warmer than the air. Let it be

* The same facility is afforded by considering cold as a body.

supposed then, that while the earth, in this state, radiates upwards a quantity of heat, a foot in depth of the incumbent air is capable of stopping a 1000th of what it hence receives, and of converting it into heat of temperature. The consequence must be that the next foot, from receiving only 999 parts of what had been emitted by the earth, will not be so much heated as the first foot, though it should absorb the same proportional quantity of what enters it. In this way, every successive foot will acquire a less quantity of heat than the preceding, and a state of the atmosphere be produced, like to that which is actually observed in a calm and sunny day. In the day, however, the phænomena, from the heating of air by rays from the earth, are somewhat confused by the warmed portions rising upwards, and mixing with what is colder; whereas, at night, the air, which has been cooled by radiating heat to the earth, is rendered, by an increase of gravity, the more fit to retain its low position. I have here, for the sake of simplifying the argument, taken no notice of the cooling of any considerable mass of the air, in consequence of the actual contact of its lowermost stratum with the earth, or by the conduction of the temperature of one portion of it to another. But, in a calm state of the atmosphere, these effects must be inconsiderable, though it appears to me impossible, in the present state of our knowledge, to determine them with any precision.

According to the view, which has been given by me of this subject, the heat of the air, in a clear and calm night, ought to increase, within the limits of the phænomon, in some decreasing geometrical ratio, as the atmosphere ascends; and this conclusion is so far confirmed, by the observations of Mr. Pictet and Mr. Six taken together, that the increase of temperature is found to be greater in a given space very near to the earth, than in an equal space more remote from it.

To return to the immediate object of this article, the fact is certain, whatever may be thought of my explanation of it, that, in every clear and still night, the air near to the earth is colder than that which is more distant from it, to the height at least of 220 feet, this being the greatest to which Mr. Six's experiments relate. If then a hill be supposed to rise from a plain, to the height of 220 feet, having upon its summit a small flat surface covered with grass; and if the atmosphere, during a calm and serene night, be admitted to be 10° warmer there, than it is near the surface of the low ground, which is a less difference, according to the observations of Mr. Six, than what sometimes occurs in such circumstances, it is manifest, that, should both the grass upon the hill, and that upon the plain, acquire a cold of 10° by radiation, the former will, notwithstanding, be 10° warmer than the latter.

But the equality here supposed to be in the cold acquired by grass, in two such situations, can seldom exist. For, according to an observation made by Aristotle,* and since frequently repeated, the air of high places is much more agitated, than that upon low ground. The frequent renewal, therefore, from this cause, of the air in contact with the grass on the hill, will prevent it from ever becoming much colder than the general mass of the atmosphere, at the same height. Consequently, any diminution in this way of the 10° of cold, formerly supposed to occur there from radiation, must be added to the difference of temperature in the grass in the two situations.

What has hitherto been said refers only to the occurrences on the very summit of the hill. With respect to its sides, these can be only a little colder than the atmosphere upon a level with them, even in its calmest state. For, in the first place, they do not enjoy the full aspect of the sky; and, in the second, the air, which is cooled by contact with them, will, from its increased gravity, slide down their declivity, and thus make room for the application of new and warm parcels to the same surface. The motion too, thus excited in the air, near to the sides of the hill, must occasion a motion in that upon the summit, which may, in some measure, account for the last-mentioned observation of Aristotle, as far as relates to what happens in a clear night.

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* Meteor. lib. 1. c. x.

The height of the hill, in this example, has been supposed to be small, to make it accord with that of the stations, whose temperatures were compared by Mr. Six with the heat of the air near the ground. But observations of the same kind will apply to hills of much greater elevation. For granting, first, that the air at the height of 220 feet is never more than 10° colder, than that near to the earth, which is not probable, and is indeed contradicted by some of Mr. Six's observations; and again, that the increase of the air's heat, in a calm and serene night, ceases precisely at the greatest height, to which Mr. Six carried his observations, which is also improbable; still a reduction, to the extent of 10°, in the temperature of the air near to the earth, will render the cold of this low portion of the atmosphere greater than that of any other portion, which is not more than 2500 or 3000 feet above the former, if the estimate be just, which makes a declension in the heat of the atmosphere of 1° for every 250, or 300 feet of its height, when no counteracting cause exists.

The remarks, however, which have been offered on the greater warmth of hills at night, in a certain state of weather, are strictly applicable to those only, which are insulated, and of inconsiderable lateral extent; and it is upon such chiefly, if not solely, that this phænomenon has been observed. The superiority of the cold of a low plain, from radiation, over that of a wide expanse of hilly ground, will, for obvious reasons, be less; and no superiority of this kind will probably exist in the former situation, when the high ground is not only extensive, but flat on the top, forming what is called a table-land; unless indeed, which seems to be actually the case, the air of such an elevated country should be commonly more agitated, than that of lower places equally level.

An explanation may be now easily given of an observation by Mr. Jefferson of Virginia,* which, however, had also been made by Aristotle,† and Plutarch,‡ that dew is much less copious on hills, than it is upon plains. For allowing, at first, the surface of the ground to be in both situations equally colder than the air which is near to it; still, as the production of dew must be in proportion to the whole depression of the temperature of the air which furnishes it, below what its heat had been in the preceding day, and as one part of this depression, the general cooling of the atmosphere, is much more considerable on the plain than on the hill, moisture must necessarily be deposited more copiously in the

* Notes on Virginia, p. 132.

† Meteor. Lib. 1. c. x.

‡ De Primo Frigido.

former than in the latter place. If the greater agitation of the atmosphere, and the less quantity of moisture, during clear weather, in its higher region than in the lower, be added, it may readily be inferred, that dew shall sometimes be altogether wanting on a hill, though abundant on a plain at its foot, agreeably to what has been actually observed by Mr. Jefferson.

IX. The leaves of trees often remain dry throughout the night, while those of grass are covered with dew. As this is a similar fact to the smallness of dew on hills, I shall in accounting for it do little more, than enumerate the circumstances on which it depends.

1. The atmosphere is several degrees warmer near the upper parts of trees on dewy nights, than close to the ground. 2. The air in the higher situation is more agitated, than that in the lower. 3. The air at a little distance from the ground, from being nearer to one of its sources of moisture, will on a calm evening contain more of it, than that which surrounds the leaves of elevated trees. 4. Only the leaves of the very tops of trees are fully exposed to the sky. 5. The declension of the leaves from an horizontal position will occasion the air, which has been cooled by them, to slide quickly away, and be succeeded by warmer parcels. 6. The length of the branches of the trees, the

tenderness of their twigs, and the pliancy of the footstalks of their leaves, will cause in the leaves an almost perpetual motion, even in states of air that may be denominated calm. I have hence frequently heard, during the stillness of night, a rustling noise in the trees, which formed one of the boundaries of the ordinary place of my observations, while the air below seemed without motion.

Nearly in the same manner is to be explained, why shrubs and bushes also receive dew more readily than lofty trees.

X. Bright metals, exposed to a clear sky in a calm night, will be less dewed on their upper surface than other solid bodies; since of all bodies they will, in such a situation, lose the smallest quantity of heat by radiation to the heavens, at the same time that they are capable of receiving, by conduction, at least as much heat as any others from the atmosphere, and more than any others from the warmer solid substances, which they happen to touch.

If the exposed pieces of metal be not very small, another reason will contribute somewhat to their being later and less dewed than other solid substances. For, in consequence of their great conducting power, dew cannot form upon them, unless their whole mass be sufficiently cold to condense the watery vapour of the atmosphere; while the same fluid will appear on a bad conductor of heat, though the parts a very little beneath the surface are warmer than the air.*

From the same ready passage of heat from one part of a metal to another, a metallic plate suspended, horizontally, in the air several feet above the ground, will be found dewed on its lower side, if the upper has become so; while the lower surface of other bodies, more attractive of dew, but worse conductors of heat, are without dew in a similar situation.

A metal placed at night in the air, near to the ground, is, for the most part, sufficiently cold to condense, on its underside, the vapour which arises from the warmer earth; though its upper surface may be dry, from possessing the same, or almost the same temperature, as the atmosphere near to it.

As the temperature of metals is never much below that of the neighbouring air, a slight diminution of their cold from radiation will often occasion them to evaporate the dew, which they had previously acquired, though other substances, which had been more cooled by radiation, are still attracting dew. For a like reason, a metal,

* I hence think it probable, that dew will sometimes form on the bulb of a thermometer, before the mercury in it is cooled below the temperature of the air. It seems certain to me, also, that dew may appear upon substances, which, from the thinness of the layer of matter their cold is confined to, will produce little or no sensible effect upon a thermometer that is applied to them.

which has been purposely wetted, will often become dry at night, while other substances are becoming moist.

A substance highly attractive of dew, such as wool, if laid upon a metal, will derive heat from it, and will therefore acquire less dew, than an equal portion of the same substance laid upon grass.

A large metallic plate will be less readily dewed while lying on grass, than if it were placed in the air, though only a few inches above the grass; because, in the former situation, it receives freely, by means of its great conducting power, heat from the earth; whereas, when placed in the air, it powerfully resists by another property, possessed in a great degree by bright metals, the entrance of heat radiated towards it by the grass beneath. Besides; the grass under the metal possesses now less heat, than when this substance was in contact with it, partly from having a small oblique aspect of the sky, and partly from receiving air, which has been cooled by passing over other grass fully exposed to the heavens.

When a piece of metal, having closely applied to its undersurface a substance of some thickness, which attracts dew powerfully, and, therefore, imbibes readily heat that is radiated to it, is exposed to the sky at night, the heat supplied by the attached substance, both from its own original store, and from what it has acquired through the radiation of the ground to it during the exposure, will enable this piece to resist longer, than a bare piece, the formation of dew, or even than another piece, which has only a thin coat of matter considerably attractive of dew attached to its underside. The experiment with the wooden cross, covered with gilt paper, affords an example of the latter fact.

A very small metallic plate, suspended in the air, is less readily dewed than a large one, similarly situated, as it receives, in proportion to its size, more heat from the atmosphere. On the other hand, a very small plate laid upon grass, rendered cold by radiation, will be sooner dewed than a larger one in the same situation, from presenting a greater proportional circumference to the surrounding grass, and therefore losing more quickly its heat by conduction. It will be also sooner dewed than another very small plate suspended in the air; since the latter, like other small bodies similarly placed, must be continually acquiring more heat than the former, in the manner described in the 86th page of this Essay.

A piece of metal, applied to different portions of cold grass in succession, will sooner become cold itself, than another piece, which is suffered to remain constantly upon one portion of the same grass, and will in consequence be sooner dewed.

If the bare side of a piece of metalled paper be exposed to a clear and calm sky at night, it will become cold, by radiation, and receive, by conduction, the heat of the inferior metallic surface; whence, if this surface be afterwards made the upper one, it will sooner acquire dew than a similar metallic surface, which has been exposed to the sky during the whole of the experiment.

When a metal covers, in part only, the upper surface of a piece of glass, the uncovered portion of the glass quickly becomes cold by radiation, on exposure to a serene sky in a still night, and then, by deriving to itself a part of the heat of the metal, occasions this body to be more readily dewed, than if the whole of the exposed surface had been metallic. In this experiment, the outer edge of the metallic surface, from being nearest to the colder glass, will be the first and the most dewed, while the parts of the uncovered glass, which are contiguous to the warmer metal, will be the last and the least dewed, of their respective substances.

A piece of glass, covered on one side with a metal, being placed on grass, with this side down, its upper surface attracts dew as readily as if no metal were attached to it; since the metal, in this situation, has no power to lessen the radiation of heat from the upper surface of the glass. I conclude, however, from general principles, for I have not made the trial, that if the same piece of glass, having its metallic side still undermost, were raised in the air a little above the grass, it would be more readily dewed on its upper surface, than if it had been without a metallic coating on the lower, as this coating must resist the introduction of heat radiated by the warmer grass, and thus preserve nearly undiminished the cold acquired, from radiation of heat to the sky, by the bare upper surface.

The preceding remarks apply to the whole class of metals; but the discoveries of Mr. Leslie, respecting the difference in the capacities of these bodies to radiate heat, furnish an explanation of a diversity among themselves, in regard to attraction for dew, which was noted in the foregoing part of this Essay. Gold, silver, copper and tin, are there said to resist the formation of dew more strongly, than other substances of the same class; but these metals, according to Mr. Leslie, radiate heat the most sparingly. On the other hand, lead, iron and steel, which, according to the same author, radiate heat more copiously than the former metals, were found by me to acquire dew more readily. I do not know, if the radiating power of platina has been ascertained by direct experiments; but, as its conducting power is small, its radiation must be great, since these qualities exist always in opposite degrees in the same substance; and I have accordingly observed it to be dewed, while the four first-mentioned metals were dry. I am ignorant both of the radiating and the conducting power of zinc, as determined by ordinary experiments; but I infer, from its being more easily dewed than gold or silver, that it radiates heat more copiously than they do; unless indeed, the pieces which I used, from having had their surfaces roughened by friction with sand, which was employed to brighten them, had acquired a radiating power, greater than that possessed by polished pieces, agreeably to the results of some of Mr. Leslie's experiments.*

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* I once intended to subjoin here an explanation of some very curious observations by Mr. Benedict Prevost on dew, which were published, first in the 44th volume of the French Annals of Chemistry, and afterwards by Mr. Peter Prevost of Geneva, in his Essay on Radiant Heat; but fearing to be very tedious, I have since given up the design. I will say, however, that, if to what is now generally known on the different modes, in which heat is communicated from one body to another, be added the two following circumstances; that substances become colder, by radiation, than the air, before they attract dew; and that bright metals, when exposed to a clear sky at night, become colder than the air much less readily than other bodies; the whole of the appearances observed by Mr. Prevost may be easily accounted for.

Note to second edition.] I found, shortly after the publication of the former edition of this Essay, that the learned Dr. Young had, several years before, in his great work on Natural Philosophy, employed the principle of the radiation of heat to account for several of the facts observed by Mr. B. Prevost. On the subject of Dr. Young's explanation, I have spoken somewhat fully in the 28th number of Dr. Thomson's Annals of Philosophy.

XI. Thinking it probable, that black bodies might radiate more heat to the sky, at night, than white, I placed upon grass, on five different evenings, equal parcels of black and white wool. On four of the succeeding mornings, the black wool was found to have acquired a little more dew than the white; whence I inferred that it had, in consequence of its colour, radiated a little more heat. But I afterwards remarked, that the white wool was somewhat coarser than the black; which circumstance alone was sufficient to occasion a difference in their quantities of moisture. Another night, I laid on the raised board a piece of paste-board covered with white paper, and close to this a second piece similar to the former in every respect, except that it was covered with paper blackened with ink. At daylight, I saw hoarfrost upon both pieces; but the black seemed to have a greater quantity than the white. A doubt, however, afterwards arose upon the accuracy of this experiment likewise; for, as the light was faint, when I viewed the two surfaces, the quantity of hoarfrost, though equal on both, might have appeared greater on the black, than on the white, from the contrast of its colour with that of the former surface. But trials of this kind, as Mr. Leslie* has observed, never afford firm conclusions; since a black body

* On Heat, p. 95.

must always differ from a white in one or more chemical properties, and this difference may of itself be competent to produce a diversity in their powers to radiate heat.

With the view to render the subject less complicated, I have hitherto treated of dew, as if it were altogether derived from watery vapour previously diffused through the atmosphere; this appearing to me to be by far its most considerable source, and none of my conclusions of any importance being liable to be affected, even by the establishment of a contrary opinion. Other writers, however, have regarded dew as being entirely the product of vapour emitted, during the night, by the earth and plants upon it. According to this theory, dew is said to *rise*.

The first trace, which I have found of the opinion, that dew rises from the earth at night, occurs in the History of the Academy of Sciences for 1687. It is mentioned there briefly and obscurely, and was, probably, shortly forgotten; for Gersten, who advanced it anew in 1733, held himself to be its author. Musschenbroek and Dufay embraced it immediately after Gersten; but the former soon admitted, that dew sometimes *falls.* As far as I have learned, no writer upon dew has since ascribed its total production to vapour, emitted by the earth at night, except Mr. Webster of New England.* But this opinion is frequently advanced in conversation by persons, not much accustomed to philosophical pursuits, chiefly, I think, because it contradicts a popular belief.

The only argument used by the French academicians, in support of their opinion, is, if I understand it rightly, that as much dew is observed under an inverted glass-bell, as in any other situation. But admitting, for a moment, this to be true, they would not thus prove, that the ground is the only source of that fluid.

Gersten was led to think, that dew rises from the earth, by often finding grass, and low shrubs, moistened with it, while trees were dry. Respecting this fact, I shall add nothing to what I have lately said upon it, But his chief argument is derived from another fact related in the first part of this Essay, which is, that a plate of metal, laid upon bare earth on a dewy night, will remain dry on its upper surface, while it becomes moist on the lower. This also is easily explicable by what has already been mentioned by me. For the lower side of the metal, in consequence of the upper being in contact

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* Mem. of American Acad. vol. III.

with the air and being exposed to a clear sky, is colder than the earth a little below the surface, and therefore condenses the vapour, which strikes against its bottom; while the upper side, from being frequently warmer, and never more than a little colder than the air, is for the most part unable to condense the watery vapour of the atmosphere.* Gersten, moreover, describes several appearances himself, which refute his opinion. He mentions, for example, that the higher parts of shrubs are more dewed than the lower; that metallic plates, placed horizontally in the air, are as much dewed on their superior, as on their inferior surfaces; and that convex and cylindrical bodies, suspended in the air, the latter having a position parallel to the horizon, are dewed only on their upper parts.

The principal reason given by Dufay for the rising of dew is, that it appears more early on bodies near to the earth, than on those which are at a greater height. But this fact readily admits of an explanation on other grounds, that have already been mentioned. 1. The lower air, on a clear and calm evening, is colder than the upper, and will,

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* I have, in like manner, observed, on a cloudy night, a piece of glass, laid over an earthen pan containing water and placed upon the ground, to be wet on its lower side, while the upper was dry; the glass being, in this situation, sufficiently cold to condense the vapour of water heated by the earth, but not enough so to condense the watery vapour of the atmosphere.

therefore, be sooner in a condition to deposit a part of its moisture. 2. It is less liable to agitation than the upper. 3. It contains more moisture than the upper, from receiving the last which has risen from the earth, in addition to what it had previously possessed, in common with other parts of the atmosphere. Dufay attempted to strengthen his argument, by exposing, on three dewy nights, similar substances at different heights from the ground, expecting that the lower would always acquire more moisture than the upper; but, upon all the nights, some one of the lower substances acquired less moisture, than some one of the higher.

Mr. Webster has advanced no new fact in favour of the opinion, of which I am speaking.

Enough having been said to prove, that dew is not entirely the product of vapour rising from the earth at night, I shall next shew, that it often occurs, when this cause can have little or no operation.

I. It appears from Hasselquist and Bruce, that in Egypt, shortly before the rising of the Nile, and consequently when the ground there is in its driest state, dew becomes exceedingly plentiful, though little or none had formed before, while the earth was somewhat less dry. The cause evidently is, as was formerly mentioned, the moist air brought from the Mediterranean by the north wind, which then prevails.

2. Mr. Webster, speaking of hoarfrost, which he properly regards as frozen dew, candidly says, though it overthrows his opinion: 'This frost appears, when the surface of the earth is sealed with frost, and of course the vapour of which it is formed cannot, at the time, perspire from the earth.'

3. I have myself, at all seasons of the year, frequently observed wool, upon the middle of the raised board, and therefore out of the way of vapour rising from the ground, to acquire more dew, than wool laid upon the grassplat.

4. The bodies, that condense the rising vapour, must necessarily be colder than it; but, as they are likewise, according to the opinion under view, of the same temperature with the air surrounding them, this also should condense the rising vapour. Dew, therefore, should never appear in any considerable quantity, without being accompanied with fog or mist. Now I can assert after much attention to this point, that the formation of the most abundant dew is consistent with a pellucid state of the atmosphere. Hasselquist makes a similar observation, with regard to Egypt; where, during the season remarkable for the most profuse dews, 'the nights,' he says, 'are as resplendent with stars, in the midst of summer, as the lightest and clearest winter nights in the north.'

But, although these facts prove, that copious dews may occur with little or no contribution by vapour immediately rising from the earth, it must yet be admitted, that some of the moisture, which forms during clear and still weather, on bodies situated upon or near its surface, is in most cases to be attributed to this source; since, in my experiments, substances on the raised board became much later moist than others on the ground, though equally cold with them. The quantity from this cause, however, can never be great. For in the first place, until the air be cooled by the substances attractive of dew, with which it comes in contact, below its point of repletion with moisture, it will be always in a condition to take up that which has been deposited upon grass, or other low bodies, by warm vapour emitted by the earth; just as the moisture formed upon a mirror by our breath is, in temperate weather, almost immediately carried away by the surrounding air. Accordingly; I have sometimes, in serene and still weather, observed dew to appear sparingly upon grass in the shade, several hours before sunset, and to continue in nearly the same quantity till about sunset, when it would increase considerably, at the time that the same fluid began to shew itself on the raised board. In the second place; though bodies situated on the ground, after they have been made sufficiently cold, by radiation, to condense the vapour of the atmosphere, will be able to retain the moisture, which they acquire by condensing the vapour of the earth; yet, before this happens, the rising vapour must have been greatly diminished, by the surface of the ground having become much colder. These considerations, added to the fact, that substances on the raised board attracted rather more dew, throughout the night, than similar substances lying on the grass, warrant me to conclude, that on nights, favourable to the production of dew, only a very small part of what occurs is owing to vapour rising from the earth; though I am acquainted with no means of determining the proportion of this part to the whole. On the other hand, however, in a cloudy night, all the dew that appears upon grass may sometimes be attributed to a condensation of the earth's vapour; since I have several times, in such nights, remarked the raised board to be dry, while the grass was moist. These nights were calm, and evaporation from the grass consequently not copious. When evaporation on cloudy nights was assisted by wind, dew has never, as was mentioned in the first Part of this Essay, been any where observed by me.*

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* The interval between the first appearance of dew in the afternoon on grass, in shaded places, and sunset, was formerly said by me, on the authority, however, of only a few observations, to be considerably greater, than that between sunrise, and the ceasing of the formation of dew upon grass in the morning. These observations were made on spots exposed during the greater

Agreeably to another opinion, the dew found upon growing vegetables is the condensed vapour of the very plants, on which it appears. But this also seems to me erroneous for several reasons. 1. Dew forms as copiously upon dead as upon living vegetable substances. 2. The transpired humour of plants will be carried away by the air which passes over them, when they are not sufficiently cold to condense the watery vapour contained in it; unless, which is almost never the case if mist does not already exist, the general mass of the atmosphere be incapable of receiving moisture in a pellucid form. Accordingly, on cloudy nights, when the air, consequently, can never be cooled more than a little below the point of repletion with moisture, by bodies in contact with it, dew is never observed upon any plants, that are elevated a few feet above the ground. 3. If a plant has become,

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part of the day to the sun. In such places, the heat acquired, from the sun, by the uppermost layer of earth, will be longer retained, than that acquired by the grass, which will, therefore, be sufficiently cool, soon after the heat of the day has declined, to condense a part of the vapour then copiously rising from the earth; whereas in the morning, both less vapour will rise, the surface of the earth having now lost a great part of its heat, and a less proportion of that which does rise will be condensed by the grass, as the temperature of this body now more nearly approaches that of the ground, from first receiving the heat of the sun reflected from the atmosphere and other substances.

by radiating its heat to the heavens, so cold, as to be enabled to bring the air in contact with it below the point of repletion with moisture, that which forms upon it, from its own transpiration, will not then, indeed, evaporate. But other moisture will, at the same time, be communicated to it by the atmosphere; and when the difference in the copiousness of these two sources is considered, it may, I think, be safely concluded, that almost the whole of the dew, which will afterwards form on the plant, must be derived from the air; more especially when the coldness of a clear night, and the general inactivity of plants in the absence of light, both lessening their transpiration, are taken into account.

An experiment, however, has been appealed to in proof, that the dew of plants actually does originate from fluid transpired by them; that namely, in which a plant, shut up in an air-tight case, becomes covered with moisture. But this experiment, if attentively examined, will be found to have little weight. First; the inclosed plant, being exempt from the cold, which its own radiation would have produced in its natural situation, on a dewy night, will transpire a greater quantity of fluid, than a similar plant exposed at the same time to the open air. Again; the small quantity of air, contained in the case, must soon be replete with moisture, after which, the whole of what is further emitted by the plant will necessarily assume the form of a fluid, whatever may be the condition of the external atmosphere; whereas, during even the clearest night, only a part of the smaller quantity of moisture, emitted by the exposed plant, will be condensed on its surface. In the last place; notwithstanding the circumstances, which favour the appearance of moisture upon inclosed plants from their own transpiration, still the quantity observed on them is said to be, for I have made no experiment myself respecting this matter, much less considerable, than what is seen upon plants of the same kind, exposed to the air for the same time, during a calm and serene night.

PART III.

OF SEVERAL APPEARANCES CONNECTED WITH DEW.

There are various occurrences in nature, which seem to me strictly allied to dew, though their relation to it be not always at first sight perceivable. The statement and explanation of several of these will form the concluding part of the present Essay.

I. I observed one morning, in winter, that the insides of the panes of glass in the windows of my bed-chamber were all of them moist, but that those, which had been covered by an inside shutter, during the night, were much more so, than others which had been uncovered. Supposing, that this diversity of appearance depended upon a difference of temperature, I applied the naked bulbs of two delicate thermometers to a covered and uncovered pane; on which I found, that the former was 3° colder than the latter. The air of the chamber, though no fire was kept in it, was at this time $11\frac{1}{2}$° warmer than that without. Similar experiments were made on many other mornings, the results of which were; that, when the warmth of the internal air exceeded that of the external, from 8° to 18°, the temperature of the covered panes would be from 1° to 5° less than that of the uncovered; that the covered were sometimes dewed, while the uncovered were dry; that at other times both were free from moisture; that the outsides of the covered and uncovered panes had similar differences with respect to heat, though not so great as those of the inner surfaces; and that no variation in the quantity of these differences was occasioned by the weather's being cloudy or fair, provided the heat of the internal air exceeded that of the external equally in both of those states of the atmosphere.

The remote reason of these differences did not immediately present itself. I soon, however, saw, that the closed shutter shielded the glass, which it covered, from the heat, that was radiated to the windows by the walls and furniture of the room, and thus kept it nearer to the temperature of the external air, than those parts could be, which, from being uncovered, received the heat emitted to them by the bodies just mentioned.

In making these experiment, I seldom observed the inside of any pane to be more than a little damped, though it might be from 8° to 12° colder than the general mass of the air in the room; while, in the open air, I had often found a great dew to form on substances, only 3° or 4° colder than the atmosphere. This at first surprised me; but the cause now seems plain. The air of the chamber had once been a portion of the external atmosphere, and had afterwards been heated, when it could receive little accession to its original moisture. It consequently required being cooled considerably, before it was even brought back to its former nearness to repletion with water; whereas the whole external air is commonly, at night, nearly replete with moisture, and therefore readily precipitates dew, on bodies only a little colder than itself.

When the air of a room is warmer than the external atmosphere, the effect of an outside shutter, on the temperature of the glass of the window will be directly opposite to what has been just stated; since it must prevent the radiation, into the atmosphere, of the heat of the chamber transmitted through the glass.

II. Count Rumford* appears to have rightly conjectured, that the inhabitants of certain hot countries, who sleep at night on the tops of their houses, are cooled, during this exposure, by the radiation of their heat to the sky; or, according to his manner of expression, by receiving frigorific rays from the heavens. Another fact of this kind seems to be the greater chill, which we often

* Phil. Trans. 1804. p. 182.

experience upon passing, at night, from the cover of a house into the open air, than might have been expected from the cold of the external atmosphere. The cause, indeed, is said to be the quickness of transition from one situation to another. But, if this were the whole reason, an equal chill would be felt in the day, when the difference, in point of heat, between the internal and external air, was the same as at night, which is not the case. Besides; if I can trust my own observation, the feeling of cold from this cause is more remarkable in a clear than in a cloudy night, and in the country, than in towns. The following appears to be the manner, in which these things are chiefly to be explained.

During the day, our bodies while in the open air, although not immediately exposed to the sun's rays, are yet constantly deriving heat from them, by means of the reflection of the atmosphere. This heat, though it produces little change on the temperature of the air which it traverses, affords us some compensation for what we radiate to the heavens. At night also, if the sky be overcast, some compensation will be made to us, both in towns and in the country, though in a less degree than during the day, as the clouds will remit towards the earth no inconsiderable quantity of heat. But on a clear night, in an open part of the country, nothing almost can be returned to us from above, in place of the heat which we radiate upwards. In towns, however, some compensation will be afforded, even on the clearest nights, for the heat which we lose in the open air, by that which is radiated to us by the surrounding buildings.

To our loss of heat by radiation, at times that we derive little compensation from the radiation of other bodies, is probably to be attributed a great part of the hurtful effects of the night air. Descartes* says that these are not owing to dew, as was the common opinion of his cotemporaries, but to the descent of certain noxious vapours, which, having exhaled from the earth during the heat of the day, are afterwards condensed by the cold of a serene night. The effects in question certainly cannot be occasioned by dew, since that fluid does not form upon a healthy human body, in temperate climates; but they may, notwithstanding, arise from the same cause, that produces dew on those substances, which do not, like the human body, possess the power of generating heat, for the supply of what they lose by radiation or any other means.

III. I had often, in the pride of half knowledge, smiled at the means frequently employed by gardeners, to protect tender plants from cold, as it appeared to me impossible, that a thin mat, or any such flimsy substance, could prevent them from attaining the temperature of the atmosphere,

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* Meteorolog. c. vi.

by which alone I thought them liable to be injured. But, when I had learned, that bodies on the surface of the earth become, during a still and serene night, colder than the atmosphere, by radiating their heat to the heavens, I perceived immediately a just reason for the practice, which I had before deemed useless. Being desirous, however, of acquiring some precise information on this subject, I fixed, perpendicularly, in the earth of a grass-plat, 4 small sticks, and over their upper extremities, which were 6 inches above the grass, and formed the corners of a square, the sides of were 2 feet long, drew tightly a very thin cambric handkerchief. In this disposition of things, therefore, nothing existed to prevent the free passage of air from the exposed grass, to that which was sheltered, except the 4 small sticks, and there was no substance to radiate heat downwards to the latter grass, except the cambric handkerchief. The temperature of the grass, which was thus shielded from the sky, was upon many nights afterwards examined by me, and was always found higher than that of neighbouring grass which was uncovered, if this was colder than the air. When the difference in temperature, between the air several feet above the ground and the unsheltered grass, did not exceed 5°, the sheltered grass was about as warm as the air. If that difference, however, exceeded 5°, the air was found to be somewhat warmer than the sheltered grass. Thus, upon one night, when fully exposed grass was 11° colder than the air, the latter was 3° warmer than the sheltered grass; and the same difference existed on another night, when the air was 14° warmer than the exposed grass. One reason for this difference, no doubt, was that the air, which passed from the exposed grass, by which it had been very much cooled, to that under the handkerchief, had deprived the latter of part of its heat; another, that the handkerchief, from being made colder than the atmosphere by the radiation of its upper surface to the heavens, would remit somewhat less heat to the grass beneath, than what it received from that substance. But still, as the sheltered grass, notwithstanding these drawbacks, was upon one night, as may be collected from the preceding relation, 8°, and upon another 11°, warmer than grass fully exposed to the sky, a sufficient reason was now obtained for the utility of a very slight shelter to plants, in averting or lessening injury from cold, on a still and serene night.

In the next place; in order to learn whether any difference would arise from placing the sheltering substance at a much greater distance from the ground, I had 4 slender posts driven perpendicularly into the soil of a grass field, and had them so disposed in other respects, that their upper ends were 6 feet above the surface, and formed the angular points of a square having sides 8 feet in length. Lastly; over the tops of the posts was thrown an old ship flag of a very loose texture. Concerning the experiments made by means of this arrangement of things, I shall only say, that they led to the conclusion, as far as the events of different nights could rightly be compared, that the higher shelter had the same efficacy with the lower, in preventing the occurrence of a cold upon the ground, in a clear night, greater than that of the atmosphere, provided the oblique aspect of the sky was equally excluded from the spots on which my thermometers were laid.

On the other hand; a difference in temperature, of some magnitude, was always observed on still and serene nights, between bodies sheltered from the sky by substances touching them, and similar bodies, which were sheltered by a substance a little above them. I found, for example, upon one night, that the warmth of grass, sheltered by a cambric handkerchief raised a few inches in the air, was 3° greater, than that of a neighbouring piece of grass, which was sheltered by a similar handkerchief actually in contact with it. On another night, the difference between the temperatures of two portions of grass, shielded in the same manner, as the two above-mentioned, from the influence of the sky, was 4°. Possibly, experience has long ago taught gardeners the superior advantage of defending tender vegetables, from the cold of clear and calm nights, by means of substances not directly touching them; though I do not recollect ever having seen any contrivance for keeping mats, or such like bodies, at a distance from the plants, which they were meant to protect.

Walls, I believe, as far as warmth is concerned, are regarded as useful, during a cold night, to the plants which touch them, or are near to them, only in two ways; first, by the mechanical shelter which they afford against cold winds, and secondly, by giving out the heat which they had acquired during the day. It appearing to me, however, that, on clear and calm nights, those on which plants frequently receive much injury from cold, walls must be beneficial in a third way, namely, by preventing, in part, the loss of heat, which they would sustain from radiation, if they were fully exposed to the sky, the following experiment was made for the purpose of determining the justness of this opinion.

A cambric handkerchief having been placed, by means of two upright sticks, perpendicularly to a grassplat, and at right angles to the course of the air, a thermometer was laid upon the grass close to the lower edge of the handkerchief, on its windward side. The thermometer thus situated was several nights compared with another lying on the same grassplat, but on a part of it fully exposed to the sky. On two of these nights, the air being clear and calm, the grass close to the handkerchief was found to be 4° warmer, than the fully exposed grass. On a third, the difference was 6°. An analogous fact is mentioned by Gersten, who says, that an horizontal surface is more abundantly dewed, than one which is perpendicular to the ground.

IV. The covering of snow, which countries in high latitudes enjoy during the winter, has been very commonly thought to be beneficial to vegetable substances on the surface of the earth, as far as their temperature is concerned, solely by protecting them from the cold of the atmosphere. But were this supposition just, the advantage of the covering would be greatly circumscribed; since the upper parts of trees and of tall shrubs are still exposed to the influence of the air. Another reason, however, is furnished for its usefulness by what has been said in this Essay; which is, that it prevents the occurrence of the cold, which bodies on the earth acquire, in addition to that of the atmosphere, by the radiation of their heat to the heavens during still and clear nights. The cause, indeed, of this additional cold does not constantly operate; but its presence, during only a few hours, might effectually destroy plants, which now pass unhurt through the winter. Again; as things are, while low vegetable productions are prevented, by their covering of snow, from becoming colder than the atmosphere in consequence of their own radiation, the parts of trees and tall shrubs, which rise above the snow, are little affected by cold from this cause. For their outermost twigs, now that they are destitute of leaves, are much smaller than the thermometers suspended by me in the air, which in this situation very seldom became more than 2° colder than the atmosphere. The larger branches too, which, if fully exposed to the sky, would become colder than the extreme parts, are, in a great degree, sheltered by them; and, in the last place, the trunks are sheltered both by the smaller and the larger parts, not to mention that the trunks must derive heat, by conduction through the roots, from the earth kept warm by the snow.*

In a similar way is partly to be explained the manner, in which a layer of earth or straw preserves vegetable matters in our own fields, from the injurious effects of cold in winter.

V. The bare mention of the subject of this article will be apt to excite ridicule, it being an attempt to show, in what way the exposure of animal substances to the moon's light promotes

* It may be remarked here, however, that a thick covering of snow, while it renders the surface of the earth warmer than it would otherwise be, must occasion the lower atmosphere to be colder, by preventing the passage of the heat of the ground to the air, either by radiation or conduction.

their putrefaction. I have no certain knowledge, that such an opinion prevails any where, at present, except in the West Indies; but I conclude, from various circumstances, that it exists also in Africa, and that it was carried thence by negro slaves to America. It was entertained, however, by persons of considerable rank and intelligence among the antients; for Pliny* affirms it to be true, and Plutarch, after making it a subject of discussion in one of his Symposia,† admits it to be well founded.

As moonbeams communicate no sensible heat to the bodies, on which they fall, it seems impossible, that they can directly promote putrefaction. But still a reason, for ascribing such a power to them, may be derived from their being received by animal substances, at the very time that a real, but generally unnoticed, cause of putrefaction, in warm climates, (and it is in these alone the opinion I am treating of has ever prevailed) is taking place, which ceases to act, as soon as the moon's light is excluded.

The nights, on which a steady moonshine occurs, must necessarily be clear; and nights, which are clear, are almost always calm.‡ A moonshiny

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* Lib. ii. §. civ.

† Lib. iii. Prob. x.

‡ Mr. De Luc has remarked, that clouds frequently dissappear

night, therefore, is one, on which dew forms plentifully; hence the expressions 'roscida' and 'rorifera luna' employed by Virgil and Statius; and hence also an opinion, held, as appears from Plutarch, even by philosophers among the antients, that the moon communicates moisture to the bodies, which are exposed to its light.*

Animal substances are among those, which acquire dew in the greatest quantity. To do this, indeed, they must previously become colder than the atmosphere; but, having acquired the moisture of dew, in addition to their own, they will, on the following day, be in that condition, which is known, by experience, to favour putrefaction most powerfully in hot climates.

The immediate cause assigned here, for the quick putrefaction of animal substances, which have been exposed to the moon's rays in a hot

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soon after sunset. *Idees sur la Meteorologie*, II. 98. I have often observed this myself, and at the same time another fact of which he takes no notice; namely, that the atmosphere is then calmer than it had been before sunset. This calmness of the air very commonly, if not always, precedes the dissipation of the clouds.

* Akin to this opinion of the antients respecting the humefying quality of the moon, is one, which has been held, by modern writers as well as antient, upon that planet's being a cause of cold to the bodies, which receive its rays; though I know of no author who has taken notice of this affinity.

country, is the same as that given by Pliny and Plutarch; but they attributed the origin of this immediate cause, the additional moisture, to the peculiar humefying quality, which they supposed that luminary to possess. This false theory has, probably, contributed to discredit, with the moderns, the circumstance which it was employed to explain.

VI. The last fact, of which I shall treat in this Essay, is the formation of ice, during the night in Bengal, while the temperature of the air is above 32°.

I have seen only two original descriptions of this process, both of which are contained in the Philosophical Transactions; the first, by Sir Robert Barker, in the 65th volume; the other in the 83rd, by Mr. Williams.

According to the method followed by Sir R. Barker's ice-maker, square excavations, 2 feet deep, and 30 wide, having been formed in a large open plain, their bottoms are covered with sugar-cane, or stems of Indian corn, dried, to the thickness of 8 inches or 1 foot. On this layer, are afterwards placed, in rows, near to each other, *small*, unglazed earthen pans, $\frac{1}{4}$ of an inch thick, and 1 inch and $\frac{1}{4}$ deep, filled with *boiled soft* water. The pans are sufficiently porous to allow their outer surface to appear moist, after water has been poured into them. Sir R. Barker adds; that the nights, the most favourable for the production of ice, are those, which are the calmest and most serene, and on which very little dew appears after midnight; that clouds and frequent changes of wind, are certain preventives of its formation; and that, although ice is thus very readily procured by art in Bengal, during the winter, it scarcely ever occurs there naturally.

The process described by Mr. Williams must, from its extent, 300 persons being employed in it, have been carried on for profit, and would, consequently, be conducted in the most economical manner. A piece of ground, nearly level, containing about 4 acres, was divided into square plats, from 4 to 5 feet wide, which were surrounded by little mounds of earth, 4 inches high. In these inclosures, previously filled with dry straw, or sugar-cane haum, were placed as many *broad*, shallow, unglazed earthen pans, containing *unboiled pump* water, as they could hold. The air was generally very still, when much ice was formed; wind prevented its formation altogether. In the morning, between 5 and 6 h., at which time alone, Mr. Williams made his observations, a thermometer, with its bulb naked, placed on the straw, amidst the freezing vessels, was never found by him lower than 35°; and he has observed ice, when a thermometer so placed was 42°. Another thermometer, suspended $5\frac{1}{2}$ feet above the ground, was *commonly* 4° higher than that among the pans. It is possible, therefore, that Mr. Williams may have seen ice, a little before sunrise, when the temperature of the air was 46°. But granting this were the fact, it would not hence follow, that the ice was formed, while the air possessed that heat. For, although the air is generally held to be in all countries colder about sunrise, than at any other time, I know from my own observations, that this is not *always* the case in England; and similar exceptions may occur in Bengal. Sir H. Davy has said, in his Elements of Chemistry, that ice will form in Bengal, when the temperature of the air is not below 50°; but he has given no authority for this assertion.

The formation of ice, in the circumstances which have been just mentioned, was attributed by Sir R. Barker altogether, and by Mr. Williams in great measure, to cold produced by evaporation. Sir R. Barker's opinion has since been adopted by some of our most distinguished writers on Natural Philosophy, as Watson, Thompson, Young, Davy and Leslie, apparently, however, without their having fully considered it, as I shall now attempt to shew.

1. It is necessary for the complete success of the process, that the air should be very still; wind, which so greatly promotes evaporation, prevents the freezing altogether. Sir R. Barker admits, that the excavations in the earth are made to in-

crease the stillness of the air in contact with the water in the pans; but, with the view to explain the utility of this stillness he supposes, in opposition to all experience, that water kept very quiet freezes more readily, when other circumstances are the same, than if it were a little agitated.

2. No proof is given, that evaporation from the pans actually does occur, at the times which are the most favourable for the appearance of ice. At any rate it cannot be considerable; since, agreeably to what is mentioned by Sir R. Barker, dew forms in a greater or less degree during the whole of the nights, the most productive of ice; and it is not to be thought, as was said upon a former occasion, that one portion of air will be depositing moisture, from possessing a superabundance of it, while another in the immediate vicinity is receiving moisture in great quantity, in the state of pellucid vapour; as the latter fact can exist only when the air is far removed from a state of repletion with water.

3. If evaporation produced the cold under consideration, the wetting of the straw or other matter, upon which the pans are placed, would tend to increase it; and, accordingly, Sir H. Davy affirms this to be the case. But Mr. Williams, who must here be regarded as the better authority, says, that it is *necessary* to the success of the process that the straw be dry; in proof of which he mentions,

that when the straw becomes wet, by accident, it is replaced; and that when he purposely wetted it in some of the inclosures, the formation of ice there was always prevented. The reasons are clear. The water, by softening the straw, renders it easily compressible by the weight of the pans, and at the same time fills up what would otherwise be vacant spaces among its parts. The straw, therefore, in this condensed state, must afford a ready passage to heat from the earth to the pans, the hinderance of which is allowed by every person to be the use of it, in this process, when dry. Again; the moisture, which passes through the straw to the earth it covers, will rise afterwards in the form of vapour, having the same temperature with the warm ground, and will communicate heat to the pans. In the last place; a part of this vapour will be condensed into water by the pans, in consequence of which heat must be extricated.

4. It is mentioned both by Sir R. Barker and Mr. Williams, in support of their opinions, that the pans, when new, are so porous, that they readily permit water to transude them; and that old pans, which permit this in a less degree, are less fit for the making of ice. But the argument, which is hence derived by them, is completely refuted by a fact related by Mr. Williams himself; for he says, that the pans are greased before they are used, to prevent the adhesion of the ice to their sides;

since, if this purpose be answered, the water can never be in contact with the pans, and therefore can never pass through them.

The real reason of the less fitness of old pans for the making of ice is perhaps the following. The production of the cold, which occurs in this process, must take place in the water; since neither the straw upon which the pans are placed, nor the air above them, was ever found by Mr. Williams of so low a temperature as 32°. Whatever, therefore, obstructs the passage of heat from the straw to the water, must favour the freezing of the latter. But this will be less effectually done by an old than by a new pan, as the density of the former is greater, from the grease forced into it by rubbing, and from the slime and sand that will enter with the water into its pores, when these are not entirely closed by the grease; which must often happen, as the smearing is performed only once in three or four days. The difference, however, in effect betwixt old and new pans must be very small; as it does not appear that the old are ever laid aside on account of their unfitness.

In a like way may be explained, without the aid of cold produced by the evaporation of moisture from the outsides of the pans, another fact mentioned by Mr. Williams, that ice was often found by him in those vessels, while water contained in a china plate, surrounded by them, had none; since the thin and dense substance of the plate must have transmitted more readily, than the thick and rare substance of the pans, the heat of the straw to the water.

5. In accounting for the making of ice in Bengal, it is requisite to shew, not only how the first film is produced, but also, in what way the thickness of this film is afterwards increased. If evaporation be the cause of this increase, it follows, that a plate of ice in the night time, and in the stillest air, both unfavourable to that process, must yet emit as much moisture, as is necessary for the production of a cold, according to Mr. Williams, of at least 14°, and according to Sir H. Davy of at least 18°; a conclusion, as it appears to me, of itself sufficient to destroy the credit of the theory, from which it is drawn.

While attending to this subject, I became desirous of acquiring some knowledge of the degree of cold, which might be produced by evaporation from water contained in a shallow vessel. With this view, I placed on a feather-bed, situated between the door and window of a room in my house in London, two china plates, into one of which as much water was poured, as covered its bottom to the depth of $\frac{1}{4}$ of an inch. The other plate was kept dry. The bulb of a small thermometer being then applied to the inside of the bottom of each plate, I observed upon many days, in various seasons of the year, the difference between these instruments while the door and window were open. I found, in consequence, that when the temperature of the air in the room was 75°, the highest at which any experiment was made, the thermometer in the plate, containing water, was between 6 and 7 degrees lower than the one in the dry plate; that the difference between these thermometers diminished gradually as the air became colder; and that when the temperature of the air was 40°, the lowest for which I have any observation, the difference was only $1\frac{1}{2}$°. At 32°, therefore, it would have been very small, and at a few degrees below 32 it would probably have vanished. This supposition agrees with an observation made by Mr. Wilson of Glasgow, who found, that no cold was produced by evaporation from snow possessing a temperature of 27°, though the air in the immediate neighbourhood was purposely much agitated by him.

The conclusions here given by me, respecting the cold produced by the evaporation of water, were drawn from experiments made in the day, while the sky was clear, the air very calm, and the temperature of the atmosphere stationary. At night, and during a cloudy day, the differences were less. On the other hand, if there was any perceptible motion in the air, they were greater. They were also greater if the heat of the atmosphere was increasing; but less, if this was decreasing.

Having thus, I expect, placed beyond doubt, that the formation of ice in Bengal is not occasioned by evaporation, I shall now state several reasons, which have induced me to believe, that it depends upon the radiation of heat to the heavens.

1. This cause not only exists, but exists in a degree, sufficient for the production of the effect, which I attribute to it. For Mr. Wilson found the surface of snow, during a clear and calm night, to be 16° colder than air 2 feet above it, the temperature of the latter being taken by a naked thermometer; whereas the greatest heat of the atmosphere ever observed by Mr. Williams, at the distance of $5\frac{1}{2}$ feet from the ground, during the time that he supposed ice to be forming, was only 14° higher than the freezing point of water. I need say nothing of the difference of 18° related by Sir H. Davy, as he does not speak from his own observation, and as he gives no authority for what he advances; though even this difference is considerably less, than what I have attempted to shew must sometimes occur, from the radiation of heat at night, between the temperature of air, a few feet above the earth, and that of bodies placed on its surface.

It is to be mentioned here also, that, according to Mr. Leslie,* the power of water to radiate heat exceeds, perhaps, that of all other substances.

2. Ice is chiefly formed in Bengal during the clearest and calmest nights; and it is on such nights that the greatest cold, from radiation, is observed on the surface of the earth. In Sir R. Barker's more refined mode of conducting the process, an unusual stillness of the air, in contact with the water to be frozen, is procured, by placing the pans containing it a little below the level of the ground; in which situation, it was formerly shewn, bodies must grow colder from radiation to the heavens at night, than in any other.

3. The cold, by means of which ice is produced in Bengal, appears, as I think may be inferred from what is said by Sir R. Barker, in its greatest degree, like cold from radiation in other substances, on those still and serene nights, during which little dew is deposited by the atmosphere.

4. Clouds and wind prevent the formation of ice in Bengal; and the same states of the atmosphere either prevent, or considerably diminish, the occurrence of cold from the radiation of heat at night by bodies on the ground.

I shall close this subject, by giving some account of a few attempts to procure the freezing of water at night, in this country, by exposing it to air of a

* On Heat, p. 80.

temperature, higher than that of 32°. These were made by me in 1812, at my usual place of experiment, which was formerly stated to be not well adapted for the appearance of a great cold from radiation, and on nights not among the most favourable to such an undertaking, even of those which occur in this country. It is proper also to mention, that I was then less able to conduct such experiments, and to make use of them, than I afterwards became, from a longer attention to similar objects.

EXPERIMENT 1st.

With a view to imitate the method of making ice described by Sir R. Barker, I had a pit dug, on the evening of the 3rd of May, in the middle of the garden so often spoken of, 4½ feet long, 3 wide and 2 deep. It consequently had the same depth as the excavations mentioned by that gentleman, but was considerably less in its other dimensions. Clean dry straw was then strewed, to the height of a foot, over the bottom of the pit. On the straw were next laid a number of small shallow earthen pans, a part of which were glazed, and a part unglazed. In the last place; all the pans were filled with soft water, which had been boiled on the same evening. Contrary to my expectation, the unglazed pans remained as dry on the outside, after water had been poured into them, as those which were glazed. I conclude, therefore, that the former were more dense in their substance, than the unglazed pans used in India; and that their density was probably the reason, why ice did not afterwards form in them, sooner than in the glazed pans, which were employed by me.

Two pans, containing boiled water, were set upon the grassplat, at a little distance from the pit. A watch-glass filled with boiled water was also placed upon the grassplat, and another was laid upon the raised board, which had been thinly covered with sand. All these arrangements were not completed before 10h. at night.

At 1h. in the morning, ice appeared in the watch-glasses on the grassplat and raised board; the heat of the air, as measured by a naked thermometer, being then, at 4 feet above the ground, $39\frac{1}{3}°$, and at 7 feet, $40\frac{1}{2}°$. At 2h. ice was observed in the pans in the pit, while a thermometer in the air, $2\frac{1}{2}$ feet above the ground, was $36\frac{1}{2}°$. Shortly afterwards, ice began also to form in the pans upon the grassplat. The temperature of grass, fully exposed to the sky, was at the same time 30°, while that of the earth an inch below the bottom of the grass was 45°. During the time of these observations dew formed copiously.

EXPERIMENT 2nd.

My next attempt was in the manner mentioned by Mr. Williams.

On the evening of the 22nd of May, I encompassed a square piece of level ground, the sides of which were 3 feet long, with a border of earth 4 inches high, and filled the area with dry straw. On this were placed several of the earthen pans, which had been formerly used, and a few smaller vessels, all containing unboiled water. After an exposure of little more than an hour, water in a watch-glass upon the straw was found frozen, the temperature of the air, 2 feet above the straw, being then 37°. In half an hour more, ice began to appear in the earthen pans, while a thermometer 5½ feet above them, this being the height at which Mr. Williams used to suspend his instrument, was 36°. The air soon after became colder; but its temperature was never less than 33°, though taken by a naked thermometer, which, as was before said, upon a clear and calm night, occasions the air to seem about 2° colder than it really is.

It might be inferred, from what is mentioned by Mr. Williams, that the temperature of the straw beds, on which the ice-pans were set at Benares, was always found by him above the freezing point, for this reason, that the straw, from containing no moisture, could not, like the water, grow cold by evaporation. I had, therefore, been surprised, during the first experiment, for I had then but little acquaintance with the phænomena of cold observed with dew, that a thermometer, laid upon an exposed part of the straw, was always below the freezing point, after ice had begun to form in the pans. On reading, however, his account of the process a second time, with increased attention, my wonder ceased. For, as the pans he speaks of were *large*, and touched one another, and as all the pans employed in India, for the making of ice, widen as they rise from the bottom, like our milk pans, the thermometer, placed by him on the straw, must have been secluded from all view of the sky, and would therefore mark a temperature much higher, than if it had been laid, as in my experiment, upon straw fully exposed to the heavens. On this, the second, night, therefore, I placed a thermometer under the edge of one of the pans lying on the straw bed, and found it some time afterwards 6° higher, than a similar instrument upon a part of the straw bed which was uncovered. Generally, however, the difference was not so great. If my pans had been large, like those of Mr. Williams, I should, no doubt, have observed more considerable differences; for, in consequence of their smallness, I could not lay a thermometer on the straw bed, so as to be fully screened from the sky by the edge of any of them, without its being almost in contact with the vesssel, every part of which was always colder than the sheltered straw.

Much dew formed in the course of this night. The greatest difference remarked by me, during it, between the temperatures of grass and of air, was 6°, and between those of air and a fully exposed part of the straw bed 9°.

EXPERIMENT 3rd.

This was begun on the evening of the 16th of October, and was likewise made agreeably to the method related by Mr. Williams.

Ice appeared in the pans, when the temperature of the air, at the height of $5\frac{1}{2}$ feet, was, according to a naked thermometer, 37°.

On this night, I placed upon the straw bed a dry earthen pan, among those which contained water, and found the inside of its bottom to be as much colder than the air, as the water was in the other pans, before ice appeared in them. After the water had begun to freeze, no proper comparison could be made between its temperature and that of the empty pan. This pan, in the course of the night, attracted moisture, which was afterwards converted into a film of ice.

But the chief fact established by the present experiment was, that water may freeze at night, in air of a temperature higher than 32°, not only without any loss of weight from evaporation, but with a gain of weight from an opposite process.

I had observed that water, exposed early in the evening in the open air to the sky, lost a little weight, in the course of a clear night. This I imputed to evaporation taking place, before the water had been cooled enough to condense the vapour of the atmosphere, and to the weight gained afterwards being insufficient to compensate the previous loss. I exposed, therefore, on this night, water to the influence of the sky, until it was cooled to 34°. Of this I put 2 ounces into each of two china saucers, which had also been exposed to the air, and then placed the saucers upon the straw bed. In the morning, a thin cake of ice was found in both saucers, one of which had gained $2\frac{1}{2}$, and the other 3 grains, in weight. Dew was also copious on this night. At one time, grass was $9\frac{1}{2}$°, and the exposed part of the straw bed 12°, colder than the air.*

* The greater cold, observed in this and the preceding experiment, upon straw than upon grass, is to be referred to the shortness of the latter, by reason of which heat was readily communicated to its upper parts by the earth.

It must be evident to every person, that the formation of ice, in the three preceding experiments, was the effect of a natural operation, similar to that by which the same substance is produced in Bengal. These two facts must, therefore, have a common cause, and this has been shewn, by the last experiment, independently of what was said before in this Essay, not to be evaporation. It is also clear, that the cold, induced on the water in those experiments, had a common cause with that observed, at the same time, upon the grass and the straw; which latter cold must, in consequence of proofs formerly given, be admitted to have arisen from the radiation of the heat of those substances to the heavens. A necessary inference, therefore, appears to be, that the formation of ice in Bengal, in the circumstances described by Sir R. Barker and Mr. Williams, must likewise be attributed, in by far the greater measure, if not altogether, to a loss of heat, which the water suffers by its own radiation, while situated in such a manner, that it can receive little heat from other bodies, either by radiation or conduction.*

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* On the evenings preceding the nights, during which ice is produced in Bengal, the temperature of the water exposed in the pans is, probably, often 60° or more. But water of the heat of 60°, if exposed in a shallow earthen vessel to air of the same temperature, during the day, while the weather is calm and clear, will lose about 3° of heat by evaporation. A cold from this cause

may, therefore, concur with that from radiation, and, consequently, may, in Bengal, accelerate somewhat the formation of ice. The influence, however, of evaporation there, in this respect, should the state of the air with regard to moisture still permit it, which must often not be the case while dew is forming, will, as the night proceeds, gradually diminish, and at length almost disappear, before the freezing of the water commences; since I have lately shewn, that evaporation from water of 32° produces very little cold, even in the day time. Indeed, it seems to me much more probable, that on a clear and calm night, though in a dry winter of Bengal, water at the temperature of 32° will acquire warmth from the formation of dew upon it, than that it will become cold from evaporation.

CONCLUSION.

The experiments which were made by me on dew, and other subjects treated of in the preceding Essay, were unavoidably attended with many inconveniences, which were the more felt, as my health had long been feeble, and as my professional duties obliged me often to return to London in the morning, without having previously taken rest, after the whole of a night had been spent in attending to the objects of my pursuit. The inconveniences here alluded to were, indeed, so great, that I was twice or thrice obliged to intermit my labours for several months together, and at length found it necessary to cease from them entirely, before I had nearly completed the plan, which I had formed. I take the liberty of mentioning these things, to excuse, in part, the imperfections, which will be observed in what I have written, as some of them would, no doubt, have been removed by a further interrogation of Nature.*

London, September 25, 1815.

* Of the experiments related in the beginning of the second Part of this Essay, with the view of proving, that the formation of dew is an effect of previous cold in the substances on which it appears, those of only one evening were remarkable for the greatness of their results, the weather upon the other evenings not having favoured much my purpose. I took advantage, therefore, of being in the country, at the distance of a few miles from

London, on the 21st of the present month, the last day but one of an unusually long tract of dry weather, to expose to the sky, 28 minutes before sunset, weighed parcels of wool and swandown, upon a smooth, unpainted, and perfectly dry fir table, 5 feet long, 3 broad, and nearly 3 in height, which had been placed an hour before, in the sunshine, in a large level grass-field. At this time, and throughout my experiments, the air was very still, and the sky very serene. The atmosphere, too, in all probability, contained but little moisture, in consequence of the long absence of rain; and the surface of the ground apparently contained none. The wool, 12 minutes after sunset, was found to be 14° colder than the air, the temperature of the latter being measured by a naked thermometer suspended 4 feet above the ground, and to have acquired no weight. The swandown, the quantity of which was much greater than that of the wool, was at the same time 13° colder than the air, and was also without any additional weight. In 20 minutes more, the swandown was $14\frac{1}{2}$° colder than the heighbouring air, and was still without any increase of its weight. My experiments now ceased from a failure of day-light.

In my former experiments of this kind, the greatest cold observed by me from radiation, without the appearance of dew, was only $9\frac{1}{4}$°.

While making the experiments on wool and swandown, I attended frequently to the temperature of the grass, and found it at one time 15° colder than that of the air 4 feet above the ground. This difference is 1° greater, than any I had ever before seen between the temperatures of the same substances, and is equal to the greatest which I had ever known to occur, between those of the atmosphere and of swandown lying upon grass. I had this evening placed no swandown upon grass.

These experiments were not made till nearly the whole of the present edition of my Essay was printed, and could not, therefore, be mentioned in their proper place.

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